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A NEW METHOD OF SHAFT-SINKING THROUGH WATER-BEARING LOOSE MATERIALS.

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Read at the Chicago Meeting of the American Institute of Mining Engineers.

In the work of exploring certain gold-bearing gravels in the American Valley, Plumas Co., California, entrusted to my charge by Prof. A. Agassiz, of Cambridge, and Q. A. Shaw, Esq., of Boston, it became necessary to sink a shaft through loose materials containing in some layers large quantities of water, and I have been compelled to devise a new method, which has proved successful and may be of service elsewhere.

The American Valley is a comparatively level tract of about 4,500 acres, surrounded with steep mountain slopes, which rise on the east, south and west to peaks of an elevation about 3,600 feet greater than that of the valley. The floor of the valley is of loose materials —gravels, sands, clays, etc.—and these rest in a rocky basin. The lip of the basin at the lowest point of its rim, where the waters leave the valley, is 165 feet higher than the bottom of it where the shaft struck the bed-rock, and the surface of the loose materials at the shaft is 45 feet higher than the lip. There is therefore, at the shaft, a thickness of 210 feet of gravels, sands and clays, resting in a bowl which receives water from a large area of mountain slopes. The shaft was sunk, by the method to be described, 20.7 feet further into the underlying bed-rock, making its whole depth 230.7 feet.

On geological grounds it was probable that the lower portions of the basin were filled with a mixture containing so much clay that water would pass through it slowly, and I tried to reach this comparatively compact material by the ordinary process of excavating and pumping out the water;* but at 14 feet below the surface the inflow of water became 67 cubic feet per minute, and was fast increasing, and the material was fine and was running in under the shoe and caving down outside of the shaft. It was evidently impracticable to sink to any considerable depth through such material, under the pressure existing when the water was pumped out of the shaft, even if the water should not exceed the practicable limits of pumping.

Exploration with drill was then made, and showed that for sixty feet the materials to be passed through were sands and gravels of a kind to run badly in places, and that below that depth, although the material, as a whole, was more

* The shaft started for this purpose is shown at A, Fig. 1 of the accompanying drawings. It was rectangular, and had an iron shoe, Aa, Fig. 1, which was pressed down with jack-screws as the excavation proceeded, and plank "cribbing" was built in as the shoe descended.

clayey, there were at intervals strata of loose, open sands and gravels as far as the drill went, which was to 170 feet.*

The depth was too great to permit the use of compressed air to balance the pressure of the water. The Kind-Chaudron process was not available, for the material would not stand unsupported for more than a few feet, in places not more than a few inches, much less for the whole depth. A caisson must be carried down with the excavation, and be kept pressed against the bottom.

The caisson adopted (B, Fig. 1 of the accompanying drawings) is a cylinder of 55 inches outside diameter, of wrought iron one-half inch thick, leaving inside diameter 4½ feet. It is made of rings 4 feet long, and the rings come together edge to edge, with edges accurately planed, and are joined together by butt-straps placed on the inside, 5 inches wide and ⅜ inch thick, to which the two adjoining rings are riveted. Each ring is of one sheet, the ends of which are accurately planed and brought together edge to edge, and connected at the vertical joint thus formed by a vertical butt-strap of the same width and thickness as the horizontal one, and, like it, placed on the inside. The caisson is, therefore, a smooth cylinder on the outside, but on the inside the butt-straps project inward one-half inch. The lower edges of the horizontal straps are chamfered, the upper edges left horizontal. The vertical straps are thinned at the end to pass under the horizontal ones, so as to add to the projection of the latter not more than three-eighths inch. The rivets are three-quarters inch in diameter, countersunk at both ends, and 2½ inches apart from center to center in the rows, and the two rows at each joint are same distance apart. The caulking was all done at the edges of the butt-straps. The lower part of the cylinder, which was to withstand the greatest pressure, was tested under a pressure, applied to the outside, of 150 pounds to the square inch.

The rings were put together in pairs where they were manufactured, making sections 8 feet long, so that there remained one horizontal row of rivets to be driven, and one horizontal seam to be caulked, to each eight feet in length of caisson, at the shaft.

At the lower end of the caisson is a shoe (Ba, Fig. 1) of rolled steel one inch thick, welded at the ends of the sheet so as to make a continuous ring without vertical seam. Its lower edge is not quite horizontal, but beveled so that the outer surface of the ring is one-quarter inch longer than the inner surface, making a cutting edge. It is scarfed to the ring next above, one-half of its thickness being cut away all around for a length of three and a-quarter inches, and the ring above lapping by for the same length, so as to rest on the shoulder of the scarf, and have its outside surface flush with that of the ring.

At the upper edge of the caisson there was always a butt-strap, projecting half its width. It would not do to bring to bear on this butt-strap the pressure necessary to force down the caisson; and a wrought-iron band (Bb, Fig. 1), an inch thick and four inches wide was put there, resting on the edge of the main sheet, and surrounding the butt-strap, and projecting 1¼ inch above the upper edges of the latter. The band was in halves, joined by bolts with nuts, as shown in the figure.

When the ground had been excavated and the caisson pressed down until its upper edge was 8 feet below the working floor, the band was taken off, another section riveted on, and the band put on top of the latter.

As the excavation proceeded, the caisson was forced down by hand with screws, reinforced toward the last by a falling weight.

The screws acted directly upon timbers 14 inches square on the end, resting on the band above mentioned. The bottom of the timbers, which rested directly on the band, was shod with 9 inches wide of wrought iron 1 inch thick. As the thread extended for only about 5 feet in length of the screws, blocking was put in between them and the lower timbers as the caisson went down, and removed when a new section was put on.

The screws, with their nuts and steps, are shown at C, Ca, Cb, Fig. 1, and need no further description, except that they were of strong cast-iron, called "gun-metal" in Boston, where they were made, and there were anti-friction buttons of hard

* There were geological data for concluding that the depth to bed-rock was not more than 250 feet.

steel in the steps. They were six in number, arranged, as shown in Fig. 1, in two sets of three each; but for the greatest part of the time only four were used, in two sets of two each. The nuts of each set were fixed into a timber 14 inches square on the end (D, Fig. 1), and this was held down against the thrust of the screws by four rods (E, Fig. 1) of round iron, 1½ inches in diameter, which passed through it and a similar timber (F, Fig. 1) placed under the main shaft-head timbers; the rods having nuts at both ends, and cast-iron washers, a foot square, between the nuts and the surface of the timbers. The shaft-head timbers bore the weight of the hoisting frame, and of a part of the building, and a load of gravel and clay, which was increased from time to time as became necessary.

One or more hydraulic presses, driven by the engine, would do the work much better, and at much less expense, than these screws. The pressure could then be kept even and constant while the excavation was going on.

The excavating was almost wholly done by a modification of the sand pump used in sinking artesian wells, which I will call the drill pump. It is shown at G, Fig. 1, and in Figs. 2, 3, 4. It is essentially a cylinder or barrel, with an annular drill at its lower edge, and a valve seated just above. It will be more fully described below. The flat drill shown in Fig. 10, was made to be used in ground too hard for the drill pump, and to break up large boulders, and was tried for these purposes a few times, but to little or no perceptible advantage. The drill-pump itself loosened the ground, broke up the boulders when too large to pass through the valve, and raised the material efficiently.

The auger stem (H, Fig. 1 and Fig. 5), jars (I, Fig. 1 and Fig. 6), sinker-bar (J, Fig. 1 and Fig. 7), rope-socket (K, Fig. 1 and Fig. 8), temper-screw and clamp (L, Fig. 1 and Fig. 9), and wrenches (Fig. 11), were the same as are used in the oil regions of Pennsylvania, where the tools were made, except that the auger-stem and sinker-bar were made shorter, and the necessary weight was secured by increased diameter, because the distance between the floor and the sheave (M, Fig. 1) was too short for tools of ordinary length.

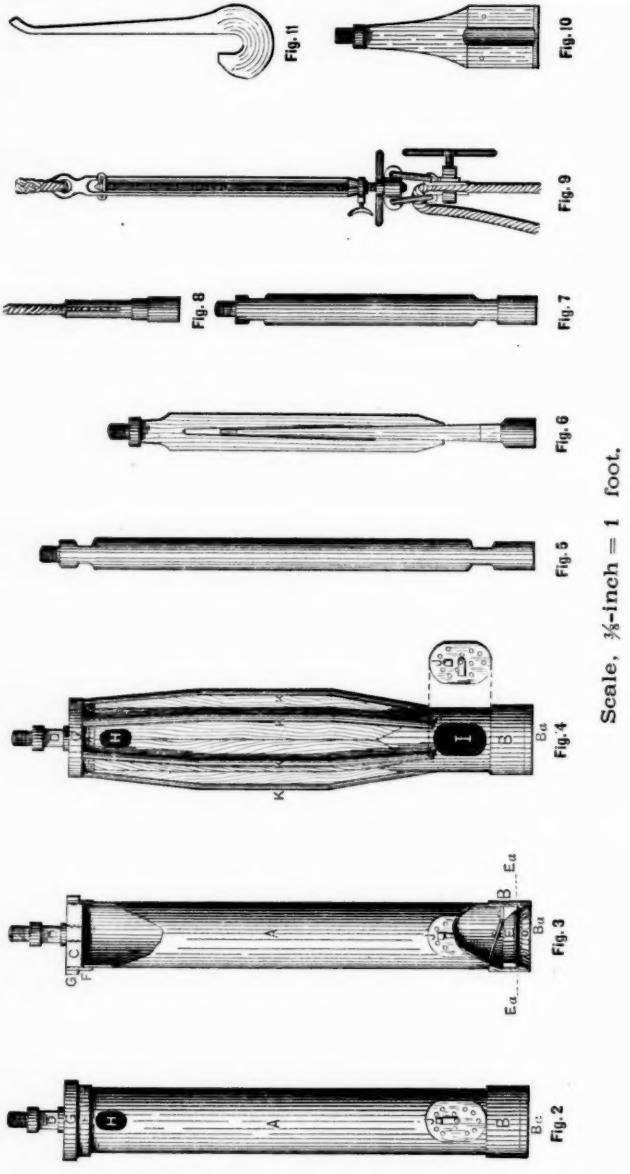
The drill pump could not have a diameter near that of the shaft, like the sand pump of artesian wells, nor could it extend (like the drills used in artesian well-boring, and in the Kind-Chaudron process) across the shaft, so as to be brought to bear on the whole area of the bottom by being turned around. It was therefore hung from a movable point, so that its position could be shifted, and it could be placed over and dropped on to any part of the bottom of the shaft. This could hardly be accomplished with the walking-beam ordinarily in use in artesian well-boring; and the drill-rope, therefore, instead of being suspended from such a beam, was connected through the clamp and temper-screw with another rope (N, Fig. 1), which was attached by a loose wooden eye (O, Fig. 1) to the wrist-pin of the gear wheel through which the drilling motion was imparted to the tools.*

This latter rope, on its way to the gear wheel, passed over a rolling horizontal drum (P, Fig. 1, and A, Figs. 12, 13, 14), and between two vertical rollers (B, Figs. 12 and 13). The drum could be moved forward by a winch (Q, Fig. 1), and run back by a weight (R, Fig. 1), and the vertical rollers could be moved to right or left by a screw (C, Figs. 12, 13 and 14), and so the point of suspension of the tools could be placed over any part of the floor of the shaft where the drill pump was to work.

The hoisting-frame and machinery were the same that were put in and are now used for ordinary hoisting and pumping, except that for pumping, a larger gear wheel has been put in the place of the one which imparted drill motion to the tools. The winding-reel (S, Fig. 1) was operated by friction gear in raising the tools and load, and controlled by break in lowering them.

The tools were let down and the connecting rope attached by the clamp La, Fig. 1, and put into place by moving the drum and rollers; the main rope slackened above the clamp, and the machinery started, giving the lift-and-drop motion to the tools. When the drill pump was loaded, the connecting rope was unclamped from the main rope and discon-

* Such a connecting rope is in common use in sinking small artesian wells by horse-power, but it passes over a sheave fixed in position.



nected with its driving wheel by slipping the wooden eye from the wrist-pin, the drum moved back, the tools raised, and the drill pump emptied.

The drill pump and its parts are shown at G, Fig. 1, and in Figs. 2, 3, 4. The whole length of the pump is 7 feet and 7 inches, besides the connecting pin (D, Figs. 2, 3, 4), which extends 10½ inches above the head. The main barrel

(A, Figs. 2,3) is of wrought iron, ¼ inch thick, with welded vertical seam, is 7 feet 1 inch long, and its inside diameter is 1 foot. The shoe (B, Figs. 2, 3, 4) screws on to the barrel at its lower end. Its cutting edge is formed by a beveled steel ring (Ba, Figs. 2, 3, 4), which is fastened made more firm by shrinking an iron ring (G, Figs. 2, 3, 4) on to the band over the edge of the plate of wrought iron; and to make the joint more secure, a similar wrought-iron plate should be put into the upper end of the barrel, and a ring shrunk on to the band over its edge.

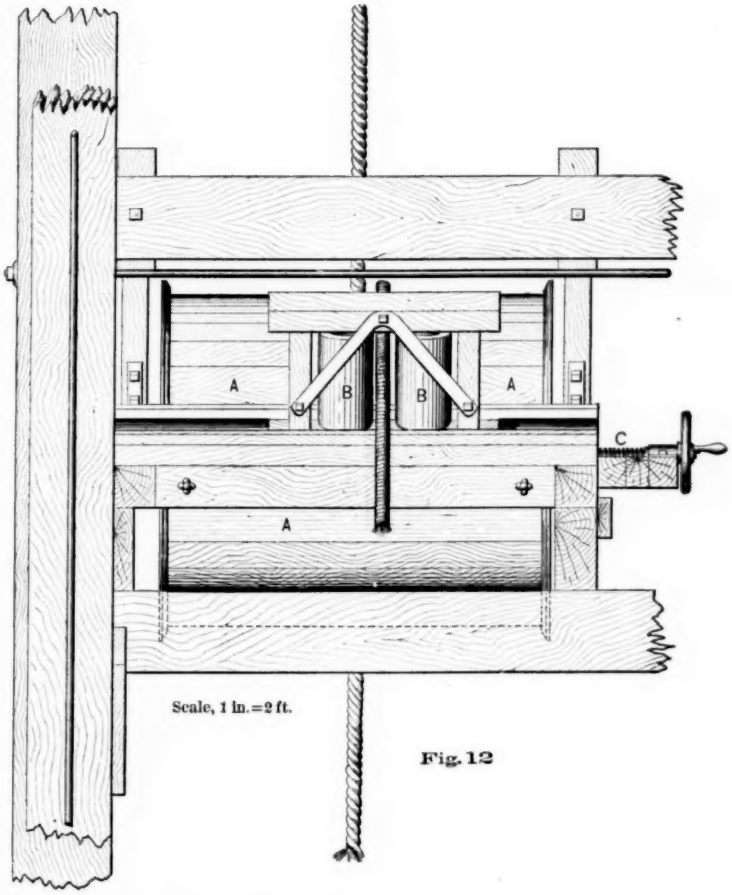


Fig. 12

with set screws to the iron of the shoe. The valve seat (E) is held in place by a projection (Ea, Fig. 3) at its lower edge, placed between the shoe and the lower edge of the main barrel. The head (C, Fig. 3) consists of a wrought-iron plate, 3 inches thick, held to the barrel by a band (F, Fig. 3) screwed on to it and on to the outside of the barrel. The joint was The pin (D, Figs. 2, 3, 4) by which the drill pump is attached to the auger-stem, is screwed into the iron plate of the head, as shown in Fig. 3. An opening (H, Figs. 1, 4) below the head let out the air as the drill pump descended into the shaft, and the water above the load during the drilling. The load was taken out of the barrel through the opening (I, Fig. 4) just

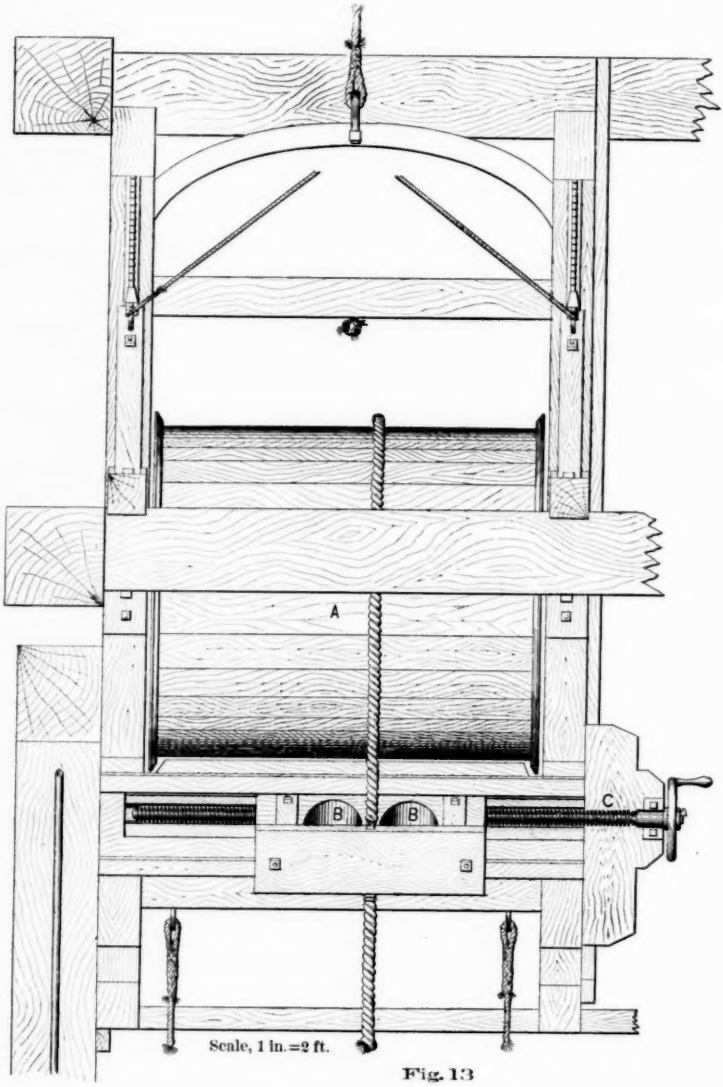
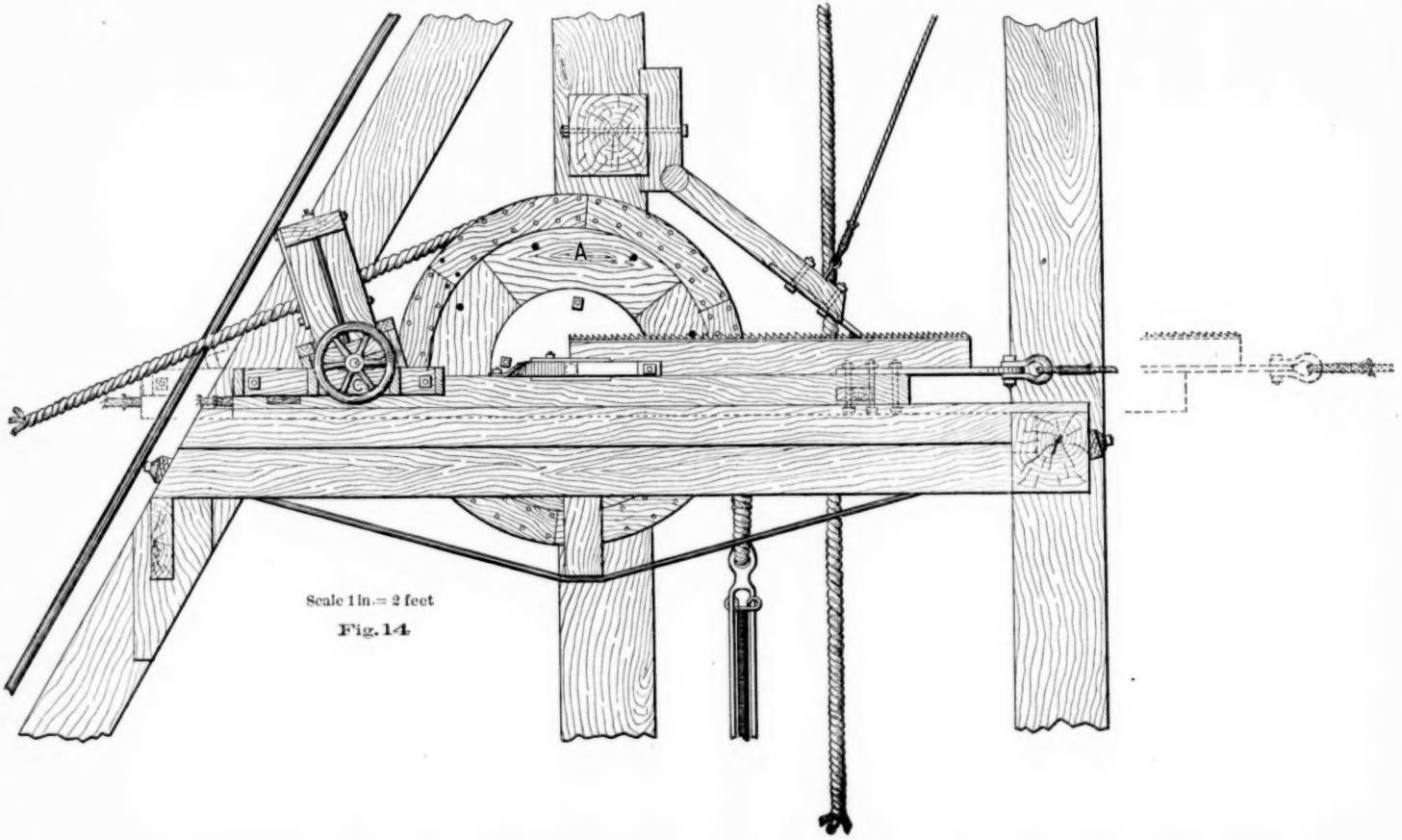


Fig. 13

over the shoe, with a small hoe made for the purpose. The opening was closed by the gate (J, Figs. 2. 3, 4). To prevent hitting the butt-straps of the caisson with the sharp edge of the shoe and the upper edge of the pump, the guard of wood and iron (K. Fig. 4) was put on. The weight of the tools as suspended was:

Pump	872½ lbs.
Auger-stem	692 "
Jars	385½ "
Sinker-bar	436 "
Rope-socket	54 "
Total	2,440 lbs.

To this is to be added the weight of



Scale 1 in. = 2 feet

Fig. 14

the rope, which increased with the depth. But the weight of the rope, rope-socket, sinker-bar and upper link of the jars added nothing directly to the force of the downward blow, and aided it only by overcoming the friction of the rope in passing through the water. In fact the sinker-bar was omitted for the greater part of the distance, but toward the last seemed to be of some service, especially when the water was thick with mud. The wood of the pump guard weighed about 72½ pounds when water-soaked, not far from the weight of the water it displaced, and neither increased nor lessened the force of the blow materially. There remained of the pump 800 pounds of iron, which, with the 692 lbs. of the auger-stem and the 193 pounds of the lower link of the jars, made a weight of iron 1,690 pounds. This, falling through water, is equal to 1,472 pounds falling through air, with some deduction to be made for the difference between the frictional resistance of air and water, and some further deduction for the greater weight and frictional resistance of the water when heavily charged with mud in suspension. The weight and consequent force of the blow increased as the pump became loaded, and at times the weight reached nearly the equivalent of a ton falling through air.

The lift and drop of the tools when drilling which was found best fitted for safe and efficient working of the machinery was 2 feet, and the number of drops 32 per minute.

When the drilling was going on, a man stood with his hand at the temper screw to let down the tools by turning the screw as the drill pump excavated, fast enough to render the blow effective, but not enough to permit the tools to topple over against the caisson. He could judge of the blow by the jar at his hand.

The length of time required to load the drill pump varied with the character and compactness of the material; but it was generally kept in motion at the bottom 20 minutes. The raising and lowering, connecting and disconnecting at the clamp and wrist-pin, taking out the gate and the load, and replacing the gate, took about ten minutes more, making 30 minutes to each charge, or two charges to the hour.

The area of the cross-section of the caisson, including its wall, is 16.5 square feet, and the area of cross-section of the drill pump 0.785 of a foot, or about $\frac{1}{21}$st of that of the caisson. The material in the drill pump was less compact than in place at the bottom of the shaft. A load of 1 foot in depth in the drill pump would lower the bottom of the shaft about ½ inch, and a load—quite often attained—of four feet in depth of the drill pump would lower the bottom of the shaft 2 inches, and 20 such loads in a day of ten hours would lower it 40 inches. The latter rate of sinking was sometimes attained, but the average rate was much lower.

The caisson was put into the rectangular shaft A, Fig. 1, which had already been sunk 14 feet, and the space outside of the caisson and within the rectangular shaft filled with clay. The only settling about the shaft observable at the surface was within the rectangular shaft. It was but little, and as it took place, clay was added at the top.

Before the work was fairly started, some days had been spent in becoming accustomed to the work and making needed alterations in the tools; and durthis time the caisson had been sunk 5 feet, or to 19 feet from the surface. The shaft and caisson were sunk the next 59.6 feet, or to 78.6 feet below the surface, in 41 working days of ten hours each, or at the rate of 1.45 feet per day. This included the time spent in putting on new sections to the caisson, which was 13.6 days.

At this depth (78.6 feet below surface), the water was taken out of the shaft. The material passed through had become more compact from about 60 feet downward, and I had expected to sink from here by ordinary methods; but the inflow of water at the bottom, which was at first 3.2 cubic feet a minute, increased in about four hours to 5.3 cubic feet per minute, and it was plainly best to continue on with the method that had succeeded so well thus far. The caisson was in good condition, whole and water-tight but slightly curved from end to end, so that the center at the bottom was about 4⅝ inches from a plumb-line dropped from the center at the top.

After a delay of some months in getting more sections of caisson, work was resumed, and the shaft was excavated and

the caisson sunk 102.4 feet further, or to 181 feet below natural surface in 91 days of ten hours each, actually spent upon the work. This included 22⅛ days spent in putting on new sections of caisson.

To this point the working force consisted of one skilled man to tend the engine, and two skilled drillers, and one or two laborers a portion of the time to aid at the screws, besides the engineer in charge. The working time was limited to ten hours a day, because the work was novel and it seemed hardly safe to have it go on in my absence, and also because I wished to observe carefully the character of the deposits passed through. The principal danger was that the excavations might be carried too far ahead of the caisson, and so cause caving and loosening of the material about the caisson. This would be obviated if hydraulic presses were substituted for screws as above suggested.

At the depth now reached (181 feet), the effect of the curve in the shaft began to be seriously felt; for the drill pump hanging vertically from the top could not be made to drop on to all parts of the floor, and left untouched a crescent-shaped area under the over-hanging portion of the cylinders. The material thus left had to be partly caved in and partly crowded in by increased pressure on the caisson. The force at the screws was increased, and consisted at times of six men; and a few weeks later the pressure of the screws was reinforced by blows of a ram consisting of a stick of timber 21 inches square and 22½ feet long, weighing, with eye-bolt, nut and washer by which it was suspended, 2,280 pounds. This was dropped two feet, thirty times a minute, on to a timber placed between the timbers on which the screws acted directly and the upper edge of the caisson.

Including the time consumed in putting on the ram, and putting on new sections, it took 23.5 days to sink the caisson the next 15.5 feet, to the depth of 196.5 feet below the surface. The excavation was 5.7 feet deeper.

The water was then again bailed out of the shaft. The curve of the shaft now left nearly half of the floor outside of the direct blow of the drill pump, and in the area thus out of reach were boulders of hard material and flattened shape, one of which was 0.44 cubic foot in bulk. These materials on the higher part of the floor were cut down by hand and thrown to the other side where the drill pump could reach them, and then taken out by the drill pump. In all, about four hours of time was spent in excavating by hand, and this was all the excavating done by hand in sinking the shaft.

At the lower edge the shoe showed a slight bulge, extending over about nine square inches of area, and projecting inwards about one-half inch at the middle of the bulge. Otherwise the caisson was unimpaired.

The inflow of water was varying, but at first averaged about 1.21 cubic feet per minute, and increased to about 2.34 cubic feet.

The next 13.5 feet brought the caisson to the surface of the bed-rock, at 210 feet below the natural surface. It took 23 days, including the time (4 days) spent in putting on two sections of caisson.

The bed-rock was a friable and rather soft clay slate, and was easily excavated with the drill pump. The caisson was sunk 20.7 feet in the bed-rock, to the depth of 230.7 feet below natural surface. To sink this 20.7 feet in bed-rock took 46 working days, including all the time of excavating, pressing caisson and putting on the sections of caisson.

After a month's delay the water was bailed out of the shaft. The caisson was in good condition, but somewhat deformed at its lower edge—not enough, however, to materially impair its usefulness. I think this deformation was caused by the bed-rock swelling and pressing against it unevenly.

The inflow of water varied, but averaged about 3.6 cubic feet per minute, or less than 5½ per cent. of the inflow at 14 feet from the surface. It came through the bed-rock clear, and was therefore not enlarging its channels.

The shaft had not only been sunk successfully through 210 feet of loose materials and 20.7 feet of rock, but the caisson had shut out the water of the upper loose gravels and sands and of the lower layers of similar materials had prevented the running in of sand, and left, to be contended with at the bottom, only a small inflow of clear water coming through rock.

A partition with ladder attached was put in as shown in Fig. 15, dividing the shaft into hoisting and pumping compartments, and a common "jack-head" pump, of 5 inches diameter and 4 feet stroke, was carried down at the same time with the partition.

A portion of the bed-rock was found loose and running, and where it was so, rods of round iron one inch in diameter and three feet long, pointed at the outer end, were driven out into the bed-rock through holes drilled through the shoe 2 inches above its lower edge, and 2 inches apart from center to center, and into the bed-rock as far as the rods were to go.

The shaft was then sunk by hand four feet below the lower edge of the shoe for a sump, and secured by staves driven around iron hoops. The staves were started about a temporary hoop of outside diameter 3 inches less than the inside diameter of the shoe, but the other hoops have the same inside diameter as the caisson, and when the staves were all in they were pushed outward and upward by the outer surface of the shoe so as to rest against it at their upper ends.

A row of iron rods like those before mentioned were driven over the space where the opening for the drift or gallery was to be made, and just above the second butt-strap counting from the bottom. The opening was cut out through the caisson between the lower edge of this butt-strap and the upper edge of the shoe. The plan of the opening and the timbers at the beginning of the drift are given in Fig. 16.

As long as the work is one of exploration, kibbles will answer for hoisting; but when necessary, a cage can be used to occupy the whole cross-section of the hoisting apartment, and when still more hoisting room is needed, another shaft can be sunk near by, and one of the two given up wholly to hoisting, and the other left for pump, ladder way and ventilating pipes; and, indeed, the capacity can be increased to any required extent by sinking a group of such shafts near one another, but with space enough between them to prevent breaking down or disturbing the ground between them while sinking. Shafts much larger than the one here described, indeed, of any ordinary size, could be sunk by the same method.

All the serious difficulty which this method of sinking encountered was caused by the curve of the caisson; and this is an avoidable difficulty. The use of hydraulic presses, as above suggested, would obviate the danger of excavating too far below the foot of the caisson, and would reduce the working force to three skilled hands.

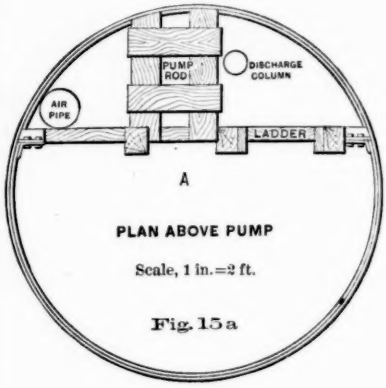


Fig. 15a

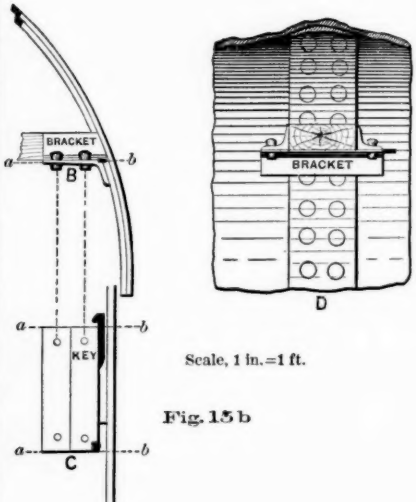


Fig. 15b

The method is not only an efficient one where ordinary methods would fail, but it is also an economical, rapid and safe method of sinking through water-bear-

ing, loose materials, and could be applied to sinking caissons for bridge piers and other foundations, as well as to mining shafts and large artesian wells.

I have given above the time actually spent in sinking. The work was not, however, done continuously, but in intervals of time spent partly in waiting for new sections of caisson to be made and transported to the ground, but principally caused by delay in ordering them. The drilling began August 21, 1882, and ended November 29, 1883. The tools were contracted for in March, 1882, and at the same time 80 feet in length of the caisson, with shoe, screws, etc. The remaining sections of caisson were ordered in several lots. The reason for not obtaining enough to go to the bottom after the experience of a few feet had proved the efficiency of the excavating tools, was the lack of data for judging of the friction to be encountered. If the caisson had been perfectly vertical, I could now present definite data for determining the friction to be overcome under similar conditions; but on account of the curve in the shaft the necessary friction on the outer surface of the caisson cannot be separated from the added resistance of the materials at the bottom which were not reached directly by the drill pump, and I can therefore add little on the subject to what is above shown, namely, that to the depth of 230 feet the friction on the outer surface of such a caisson carried through such materials is overcome with a pressure easily applied and safely within the endurance of a wrought-iron cylinder, having the thickness of iron proportioned to area of its cross-section as in the one above described, and having a shoe of rolled steel, proportioned as the one described.

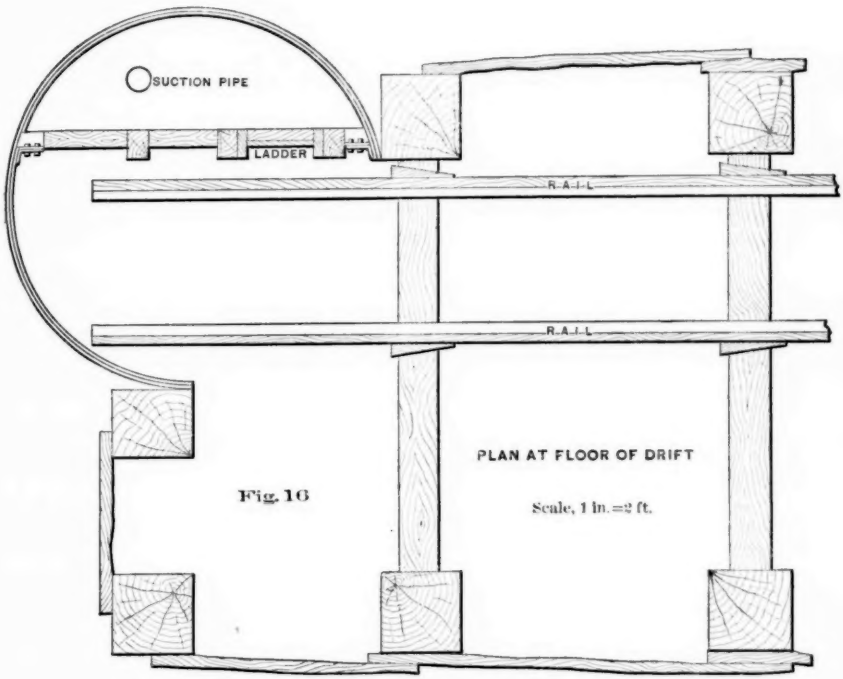


Fig. 16

INCANDESCENT LAMP ECONOMY.

By Assistant Engineer W. D. WEAVER, U. S. N.

Written for Van Nostrand's Engineering Magazine.

The cost of a given light from an incandescent lamp depends upon three quantities: the cost of the lamp, its life, and the cost of the energy to produce the light, the latter value being the entire cost of the energy actually delivered at the lamp, depreciation of plant and interest on capital being taken into consideration. The relation between these quantities may be formulated as follows:

Let a=the entire cost of one horse-power per hour *at the lamp.*

b=cost of lamp.

l=life of lamp.

x=candle power.

Then $\frac{b}{lx}$ is the hourly *lamp* cost of one candle power,

$\frac{CE}{745}\times a\times\frac{1}{x}$ is the hourly *energy* cost of the same,

and $\frac{b}{lx}+\frac{a\,CE}{745\,x}$ is the *entire* hourly cost of the unit of light.

If now the relation between the electrical energy transformed in a lamp and the resulting candle power were known, the economic conditions in respect to the life and cost of lamp and cost of horse power, could be determined by means of the above formula.

There is, however, a great diversity of opinion in regard to the law connecting these two variables. Dr. Higgs states that the candle power varies as the fourth power of the current, or as the square of the energy; Dr. Hagen (Dresden) gives the powers as five and three, respectively, while Mr. Preece, in a paper read before the British Association at Montreal, gave the law that the candle power varies as the sixth power of the current.

The plotted curve has evidently the form of curves represented by the equation $y=kx^n$, and its regularity suggests that the law is not so variable as to give reason for the diversity of values hitherto given, a conclusion justified by the result of the following investigation:

I.

In the calculations the data used were those of the careful experiments made at the Crystal Palace Electrical Exhibition, contained in an able report on the exhibition made to the Navy Department by Mr. F. J. Sprague, late U. S. N.

The values of n in the formula $y=kx^n$ were calculated for both energy and current by means of the following equations, in which P is the candle power, C the current, and E the electro-motive force between the lamp terminals:

$$n'=\frac{\log. P_2-\log. P_1}{\log. C_2+\log. E_2-\log. C_1-\log. E_1}$$

$$n''=\frac{\log. P_2-\log. P_1}{\log. C_2-\log. C_1}$$

The data used were those from experiments on 18 lamps,—6 Edison, 6 Swan, and 6 Maxim; the value of n was calculated for each successive experiment between about 5 candle power and the highest power noted that gave satisfactory results, and also for the extreme values of the candle power for each lamp.

The results are as given in the following table:

	Number of Determinations.	n' ($P=KC\overline{E}^{n'}$)	n'' ($P=KC^{n''}$)
Mean for successive values of candle power...	51	2.72	4.64
Mean for extreme values of candle power.....	18	2.71	4.70

Adopting as the exponent of the energy the value 2.70, the probable error with the above data is between +.07 and −.03. The variation of the value of the exponent of the current is much greater, however, and, adopting the value 4.70, the probable error is ±.30.

It would appear that n has a slightly greater value for the large filament or Maxim lamp than for the lamps with finer filaments; thus, with the former the mean values of n' and n'' are 2.75 and 4.97 respectively, and the variation small; this difference may be owing, however, to the more satisfactory data of the Maxim lamp, due to its larger range of candle-power.

Although the above values of n are probably correct within the limits given, other determinations should be made with smaller increments of candle power than in the experiments at the Crystal Palace, and the variation of the exponent n, if any, determined for different-sized filaments.

The following table will show the agreement of the calculated with the observed candle power. The figures at the head of the columns are from the report on the Crystal Palace Exhibition, and were taken by the committee from curves plotted with observed data. The other values were calculated from the equation $CE = K P^{\frac{1}{2.7}}$ in which k was determined for the normal candle power.

From the data of experiments made at the Munich Exhibition on the Gatehouse lamp, the following table was calculated, using the formula $C E = K P^{\frac{1}{2.7}}$:

Lamp.	Normal Candle Power.	k.	Candle Power.						
			5	10	15	20	30	60	90
Swan.....	20	21.36	5.52	10.40	15.30	20	30.18	61.51	97.60
Maxim....	20	26.98	6.18	10.67	15.17	20	29.95	58.30	94.63
Edison....	16	30.26	5.92	10.42	15	19.7	30.24	62.37	102.6
Observed C. P..		5.89	8.40	11.12	15.3	21.93	30.76	41.52	
Calculated C. P.		6.87	9.01	11.67	15.3	20.3	27.3	38.3	
Differences.....		.98	.61	.55	0	−19.3	−3.46	−3.12	

As might have been expected from the difference of construction of the lamp, the agreement is not very close, and the differences show that the equation to the curve of this lamp is of a less degree, and by calculation from the data given was found to be $CE = K P^{\frac{1}{3}}$, and the constant k, 25.58.

II.

Knowing now the relation between the candle power and energy, we will return to our first formula,

$$\frac{b}{l x} + \frac{a C E}{745 x} \qquad (1)$$

Expressing the law just deduced in the same notation, and substituting for CE in (1), we have finally for the whole cost of the unit light

$$y = \frac{b}{l x} + \frac{a k x^{\frac{1}{2.7}}}{745 x} \qquad (2)$$

With this equation, some interesting problems in incandescent lighting may be solved, and from it the following tables were deduced, showing the economic relation between the life of a lamp, its cost, and the cost of the energy furnished it. Table I. was constructed by assuming the life of a normal 20 candle

TABLE I.

$a=1$.

Total cost of unit light per hour, $y=\frac{100}{20\times1000}+\frac{1\times60}{745\times20}=.00902$.

Candle Power.	Cost of hourly H. P.	Watts.						
		2	2.5	3	3.5	4	4.5	5
10	1	2052	2607	3577	—	—	neg.	neg
	2	—	neg.	neg.	neg.	neg.	neg.	neg.
	3	neg.	neg.	neg.	neg.	neg.	neg.	neg.
20	1	792	881	**1000**	1155	1364	1674	2160
	2	1367	2160	5138	neg.	neg.	neg.	neg.
	3	5138	neg.	neg.	neg.	neg.	neg.	neg.
30	1	479	518	564	618	684	766	869
	2	684	870	1195	1907	4706	neg.	neg.
	3	1195	2720	neg.	neg.	neg.	neg.	neg.
40	1	320	364	389	417	449	489	533
	2	420	533	654	846	1198	2132	—
	3	654	992	2053	neg.	neg.	neg.	neg.
50	1	266	280	295	313	334	355	380
	2	332	380	444	533	680	892	1344
	3	462	593	892	1801	neg.	neg.	neg.
60	1	216	226	237	249	262	276	292
	2	262	293	332	383	452	552	706
	3	332	414	552	827	1680	—	neg.
70	1	183	190	198	207	217	227	239
	2	216	239	266	300	344	403	488
	3	266	320	403	544	837	1806	neg.
80	1	158	163	170	177	185	192	200
	2	184	200	220	245	281	314	365
	3	221	259	313	398	541	858	2026
90	1	139	144	148	154	159	166	172
	2	160	173	188	207	228	255	290
	3	188	216	255	314	399	554	908
100	1	124	128	132	136	141	147	151
	2	141	151	163	178	195	218	240
	3	163	186	215	256	314	422	581

power lamp to be 1,000 hours, and that the lamp *when working at its normal capacity* would produce the normal light at the rate of 3 watts per candle power, the cost of the lamp being taken at $1.00, and the cost of an hourly horse-power at 1 cent; from these data y was calculated and the life of the same lamp at other candle powers, and of other lamps of different efficiencies and at different candle powers was found, the cost of the unit light being the same in each case and as deduced from the above data. The watts at the head of the columns represent the efficiency of the lamp, and are the number of watts required to produce the unit light when the lamp is being used at its normal power—20 candle power in each case; the figures 1, 2, 3 to the right of each candle power are the respective costs of an hourly horse-power, and enable a comparison to be made between the efficiencies at these different values, the cost of the unit light and of the lamp remaining the same. The other quantities are the lives that would have to be ob-

TABLE II.

$a=2$.

Total cost of unit light per hour, $y=.01305$.

Candle Power.	Cost of hourly H. P.	Watts.						
		2	2.5	3	3.5	4	4.5	5
10	1	1195	1272	1466	1724	2106	2696	3882
	2	2095	3822	—	neg.	neg.	neg.	neg.
	3	neg.	neg.	neg.	neg.	neg.	neg.	neg.
20	1	482	515	554	598	650	713	788
	2	650	788	**1000**	1867	2160	—	neg.
	3	1000	1674	3820	neg.	neg.	neg.	neg.
30	1	303	318	335	354	374	398	424
	2	374	424	489	577	704	902	1255
	3	473	634	902	1529	—	neg.	neg.
40	1	220	230	239	249	260	273	286
	2	261	286	318	358	409	480	571
	3	318	382	476	634	946	1908	—
50	1	173	179	185	192	199	207	215
	2	200	215	234	257	287	319	362
	3	235	270	319	410	510	695	1146
60	1	142	146	150	155	160	165	171
	2	160	171	184	199	216	236	267
	3	184	207	236	276	332	412	546
70	1	120	123	127	130	134	138	142
	2	134	142	152	162	174	186	205
	3	152	168	188	215	249	296	366
80	1	104	107	109	112	116	118	122
	2	115	121	128	136	145	156	167
	3	129	141	155	174	197	228	282
90	1	94	95	96	99	101	103	106
	2	101	106	111	118	125	132	141
	3	111	121	132	147	163	184	224
100	1	82	84	86	88	90	92	94
	2	90	94	98	103	109	116	122
	3	98	106	115	126	138	156	174

tained in order that the cost of the unit light may remain the same as given for each table and therefore represent the relative efficiencies of different lamps at different candle powers and at the given constant cost of the unit of light. Table II. was calculated in the same manner, but assuming the cost, y, of the unit light to be that resulting when the energy costs 2 cents per hourly horse-power, and in Table III. the latter value is taken at 3 cents.

For any other cost b' of a lamp, the quantities in each table should be multiplied by the ratio $\frac{b'}{100}$.

If a relation could be established between different candle powers of a lamp and the resulting lives, by substitution in (2) and differentiation, the value of the candle power could be obtained at which the cost of the unit light is a minimum. It is probable that an equation of the form $l=k(m-x)^n$ would approximately express this relation, m being the candle power at which the filament is practically

TABLE III.

$a=3$.

Total cost of unit light per hour=.01708.

Candle power.	Cost of hourly H. P.	Watts.						
		2	2.5	3	3.5	4	4.5	5
10	1	798	841	927	1019	1137	1293	1496
	2	1136	1505	2165	3246	—	neg.	neg.
	3	5163	—	neg.	neg.	neg.	neg.	neg.
20	1	347	364	383	403	427	452	482
	2	426	482	554	650	788	1000	1367
	3	554	713	**1000**	1674	5138	neg.	neg.
30	1	220	230	238	248	258	269	280
	2	258	280	307	340	380	431	498
	3	307	359	431	537	724	1095	2247
40	1	165	167	172	178	183	189	196
	2	183	196	210	227	246	270	297
	3	210	236	269	313	375	468	614
50	1	128	131	135	138	142	146	150
	2	142	150	159	169	182	194	209
	3	160	175	194	224	252	289	346
60	1	105	108	110	113	115	118	121
	2	115	121	127	134	142	150	160
	3	127	138	150	165	184	206	235
70	1	80	91	93	95	97	99	102
	2	97	101	106	111	117	123	130
	3	106	114	123	133	146	161	180
80	1	78	79	81	82	84	85	87
	2	84	87	91	95	99	103	109
	3	91	97	104	108	120	131	144
85	1	69	70	71	72	74	75	76
	2	74	76	79	82	86	89	93
	3	79	84	89	95	102	110	123
90	1	62	63	64	65	66	67	68
	2	66	68	70	73	75	79	81
	3	70	74	78	83	89	96	102

destroyed. As this important question from its commercial aspect appeals with especial force to those working large installations who have also the necessary facilities for making the experiments, it is probable that this final problem will soon be solved.

A PORTABLE MINING STORAGE BATTERY.—At a recent meeting of the Engineers' Club of Philadelphia, Mr. C. Henry Roney exhibited a portable storage battery for mining and exploring purposes, with small incandescent lamps. The battery was a modification of Plante's, devised by Dr. E. T. Starr, of Philadelphia, the electrodes consisting of V-shaped plates of sheet lead arranged over each other, the convexity downward, with a slight interval between them, their ends being attached to a lead frame by burned joints, the interstices between the plates being filled with finely divided metallic lead, exposing a large surface to oxidation and reduction when subjected to dynamic electric or voltaic energy, and, in turn, giving off a large percentage of the stored energy to incandescent lamps placed in the circuit. The battery is $3\frac{1}{4}$ inches long, $2\frac{3}{4}$ inches high, and $\frac{3}{4}$ inch thick, and is said to contain a small two-candle incandescent lamp at incandescence for about one hour. A battery sufficiently large to run an eight-candle lamp for ten or twelve hours would, consequently, not be too large or heavy to carry conveniently for mine or other underground exploration.

POSITIONS OF LIVE LOAD GIVING MAXIMUM STRAINS FOR SINGLE INTERSECTION TRUSSES—ANALYTICAL SOLUTION.

By WM. CAIN, C. E., South Carolina Military Academy, Charleston, S. C.

Contributed to Van Nostrand's Engineering Magazine.

1. The live load assumed below consists of two "consolidation" engines coupled, followed by cars weighing 2,240 lbs. per linear foot, the live load being concentrated upon points as given in the diagram on page 194.

The successive wheel loads are numbered from the left to the right in order w_1, w_2, ... up to the uniform car load.

The center of gravity of the two locomotives and tenders is easily found to be 49.25 feet from w_1 (the front wheel), and 54.15 feet from the front of the car load of 2,240 lbs. per foot.

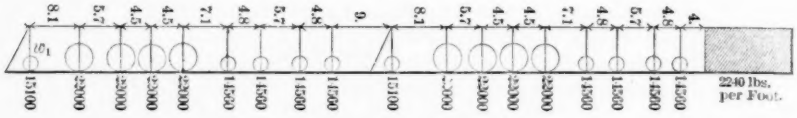
2. *For one truss*, the weights are ½ the above. The following table gives the successive weights for one truss, together with the total distances from w_1 and the total weights up to certain points.

The following little table of weights for feet and tenths of the uniform load of 1,120 lbs. per foot will prove a great convenience, in connection with the previous table, in rapidly estimating for any given length of load the corresponding weight.

Distance from w_1.	Load.	Total Loads.	Distance from w_1.	Total Loads.	Distance from w_1.	Total Loads.
Feet.	Lbs.	Lbs.	Feet.	Lbs.	Feet.	Lbs.
0	w_1 = 7,550	7,550	104	162,010	270	347,930
8.1	w_2 =11,000	18,550	106	164,250	280	359,130
13.8	w_3 =11,000	29,550	108	166,490	290	370,330
18.3	w_4 =11,000	40,550	110	168,730	300	381,530
22 8	w_5 =11,000	51,550	120	179,930	310	392,730
29.9	w_6 = 7,280	58,830	130	191,130	320	403,930
34.7	w_7 = 7,280	66,110	140	202,330	330	415,130
40.4	w_8 = 7,280	73,390	150	213,530	340	426,330
45.2	w_9 = 7,280	80,670	160	224,730	350	437,530
54.2	w_{10}= 7,550	88,220	170	235,930	360	448,730
62.3	w_{11}=11,000	99,220	180	247,130	370	459,930
68.0	w_{12}=11,000	110,220	190	258,330	380	471,130
72.5	w_{13}=11,000	121,220	200	269,530	390	482,330
77.0	w_{14}=11,000	132,220	210	280,730	400	493,530
84.1	w_{15}= 7,280	139,500	220	291,930	410	504,730
88.9	w_{16}= 7,280	146,780	230	303,130	420	515,930
94.6	w_{17}= 7,280	154,060	240	314,330	430	527,130
99.4	w_{18}= 7,280	161,340	250	325,530	440	538,330
103.4	——	161,340	260	336,730	450	549,530

Feet.	0	.1	.2	.3	.4	.5	.6	.7	.8	.9
0	0	112	224	336	448	560	672	784	896	1008
1	1120	1232	1344	1456	1568	1680	1792	1904	2016	2128
2	2240	2352	2464	2576	2688	2800	2912	3024	3136	3248
3	3360	3472	3584	3696	3808	3920	4032	4144	4256	4368
4	4480	4592	4704	4816	4928	5040	5152	5264	5376	5488
5	5600	5712	5824	5936	6048	6160	6272	6384	6496	6608
6	6720	6832	6944	7056	7168	7280	7392	7504	7616	7728
7	7840	7952	8064	8176	8288	8400	8512	8624	8736	8848
8	8960	9072	9184	9296	9408	9520	9632	9744	9856	9968
9	10080	10192	10304	10416	10528	10640	10752	10864	10976	11088

Fig. 1



POSITION OF LIVE LOAD CAUSING MAXIMUM MOMENTS.

3. The following investigation applies either to a beam or to a single intersection truss, with vertical members at each apex.

In the figure,

P $=(w_1+w_2+\ldots+w_n)=$ sum of live loads to left of B.

R =sum of total live loads on span when w_n is at B.

M =moment, when w_n is at B, about B.

M′=moment, when w_n is x to left of B, about B.

px=new load moving on as load is shifted x to left.

d =distance from B to left abutment.

l =length of span, a= distance from w_n to w_{n+1}.

After the shifting, which moves w_n from B, to a point x to left of B, the left reaction V is increased by

$$\mathrm{R}\frac{x}{l}+\frac{px^2}{2l}$$

and its moment by

$$d\left(\mathrm{R}\frac{x}{l}+\frac{px^2}{2l}\right).$$

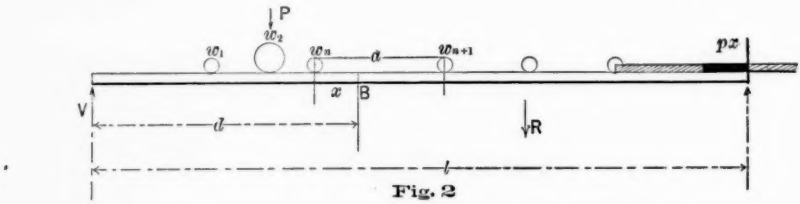


Fig. 2

The downward moment of P is increased by Px.

$$\therefore \mathrm{M}'=\mathrm{M}+\frac{a}{l}\left[\mathrm{R}+\frac{px}{2}-\mathrm{P}\frac{l}{d}\right]x \quad . \quad . \quad (1)$$

First. So long as the [ ] is + for $x=a$, the moment is increased by shifting the loads a to left, so that w_{n+1} rests at B, when by the same method we ascertain whether the moment is increased on shifting loads still further to left, so that w_{n+2} rests at B, and so on.

The [ ] may be minus for some value of x less than a, still if it is positive for $x=a$, M′ > M and the moment is increased by the shifting.

Second. When however, P is increased, so that the [ ] becomes minus, even for $x=a$, M′ is greatest for $x=o$, or w_n at B, w_n being the weight just to left of B.

4. It is best to assume, at the start, such a position of the live load that the [ ] is plus, when as the loads shift to the left and new loads pass to the left of B the [ ] will eventually become minus. The instant this occurs, the nearest load on the left of B is the one that must rest at B to give the maximum moment at that point. The [ ] changes sign when

$$\frac{\mathrm{P}}{\mathrm{R}+\frac{pa}{2}}=\frac{d}{l}$$

or the ratio of the loads on the left of B to the whole load on the span, approximately, is equal to the ratio of the segment of the span to the left of B, to the whole span. This principle will indicate roughly how to place the loads for a first trial.

5. If the *new load* moving on the span is an *isolated wheel load* Q, whose distance from the right abutment is λ, when $x=a$; then the moment caused by it is

$$\frac{\mathrm{Q}\lambda}{l}d=\frac{d}{l}\left(\frac{\mathrm{Q}\lambda}{a}\right)a$$

which replaces the term $\frac{d}{l}\left(\frac{px}{2}\right)x$ in the formula. In this case so long as

$$\left(\mathrm{R}+\frac{\mathrm{Q}\lambda}{a}-\mathrm{P}\frac{l}{d}\right)$$

remains +, the live load is shifted to the left; when it changes to minus, the last load supposed to the left of B is then placed at B for a maximum moment.

It is not always necessary to compute the terms involving the new loads brought on the span by the shifting, as the proper conclusion can generally be derived without it. Still the labor of including these terms, when necessary, is trivial.

6. It is to be remembered that the above investigation applies exactly to a beam as well as to trusses, like the Pratt-Howe, triangular with suspenders or vertical posts at each apex, and in fact to any single system, where the moment about the apex B is the same as for a beam. It is obvious that in getting the moment for these trusses, that it is immaterial whether the loads are taken as they stand on the rails or as concentrated at the apices of the truss, for the moment of a resultant equals the sum of the moments of its components. For *all web members* inclined however, the downward moment about an apex, is generally different in the two cases. Thus in the Warren girder, if there is any live load on the chord piece under investigation, say to the right of B, it is not included in the above formula in the downward moment, whereas a part of this load is held up at the apex to the left of B, and hence should be so included. In fact, for a load placed at the middle of the chord piece, one-half of it is held at the left apex. We see, therefore, that the method is inapplicable to the loaded chord of a Warren girder, though it applies to the other chord and is probably sufficiently near in practice for both chords.

7. It is seen from the foregoing that we propose examining each apex in turn for its maximum moment. Where no new loads come on or move off during the entire shifting, we could at once find the *segments* of the span commanded by any particular wheel. The first analytical solution of this case, was given by the writer in this magazine, in the August, 1878, number, page 149 (also see *Van Nostrand's Science Series*, No. 38, page 69). This solution is in fact identical in its results with the one just exposed, except in the influence of new loads moving on—generally very small, though easily included.

8. To show the practical working of the above method, let us consider a span of 200 feet divided into 10 panels of 20 feet each. The apices are lettered, A, B, . . ., K, beginning at the left abutment. The results are put in tabular form for convenience; the first column gives the apex and the load causing the maximum moment about it when found, the next column gives w_n or the load first supposed at the apex and afterwards a distance a to left of the apex.

The length of R when w_n is at apex, value of a, the distance from w_n to w_{n+1}, weight of R, $\frac{pa}{2}$ and P, which follow in the remaining columns, are all quickly obtained from the two tables of loads given above.

From the discussion above when

$$\left(\mathrm{R}+\frac{\mathrm{P}a}{2}-\mathrm{P}\frac{l}{d}\right)$$

changes from + to −, the weight w_n corresponding to the last value is the one that should rest at the apex considered for maximum moment about that apex. As soon as found this weight is put under the corresponding apex in first column.

The weights have been written in full, though it suffices generally to express them to the nearest thousand pounds. The term $\frac{pa}{2}$ need not be computed except in doubtful cases. When the new load moving on is an isolated one, we replace $\frac{pa}{2}$ by $\frac{\mathrm{Q}\lambda}{a}$ as before explained.

A very short time will suffice to make out a table like the foregoing, so that the analytical method applied to this case is seen to be practical, and since it is absolutely correct in theory, it should prove satisfactory.

As the examination of a span of any length for maximum moments is made in exactly the same manner, it is not necessary to give other examples of the application of the method.

Apex.	w_n.	Length of R in feet.	a.	$R+\frac{pa}{2}$	$P\frac{l}{d}$
B	w_2	188.1	5.7	256,200+3,192>	$(w_1+w_2)10$=185,500
w_3	w_3	193.8	4.5	262,590+2,520<	$(w_1+..+w_3)10$=295,500
C	w_4	178.3	4.5	245,230+2,520>	$(w_1+..+w_4)'5$=202,750
w_5	w_5	182.8	7.1	250,270+3,976<	(1———5) 5=257,750
D	w_7	174.7	5.7	241,190+3,190>	(1———7)$\frac{1}{3}$°=220,370
	w_8	180.4	4.8	247,580+2,688>	(1———8)$\frac{1}{3}$°=244,630
w_9	w_9	185.2	9.	252,950+5,040<	(1———9)$\frac{1}{3}$°=268,900
E	w_{11}	182.3	5.7	249,706+31,92>	(1———11)$\frac{1}{4}$°=248,100
w_{12}	w_{12}	188.	4.5	256,090+2,520<	(1———12)$\frac{1}{4}$°=275,550
F	w_{13}	172.5	4.5	238,730+2,520<	(1———13)2=242,440
w_{13}	w_{13}	168.	4.5	233,690+2,520>	(1———12)2=220,440

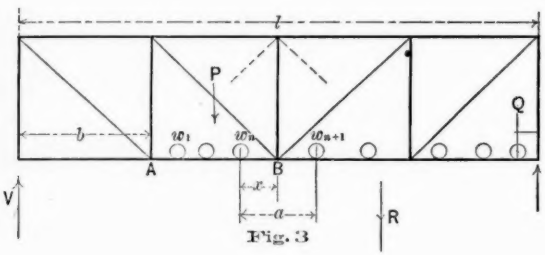


Fig. 3

POSITION OF LIVE LOAD GIVING MAXIMUM SHEAR.

9. In the following figure call

l = *length of span.*

b = *length of panel.*

$P=w_1+n\ldots+w_n$ = *sum of live loads to left of* B.

R = *sum of live loads on span when* w_n *is at* B.

a = *distance from* w_n *to* w_{n+1}.

It will first be supposed that none of the loads are to the left of A, also that no new loads move on the span as the above load system is shifted to the left.

Let us take the position of the loads such that w_n is at B. If now we shift the entire system to the left of a distance x, the left reaction V, is increased by $R\frac{x}{l}$ and the reaction at A, or part of the loads ($P=w_1+\ldots+w_n$), held at A, is increased by $P\frac{x}{b}$; so that the *increase in the shear* is

$$S-S'=\frac{1}{l}\left[R-P\frac{l}{b}\right]x. \quad . \quad (1)$$

10. In case the [ ] is +, the shear is increased by the shifting to the left, otherwise it is decreased, in which last case w_n remains at B. When however the [ ] is +, $S-S'$ increases with x and is greatest when $x=a$, which moves w_{n+1} up to B at least. Other positions should similarly be tried, when it is very quickly determined what weight should rest at, or command,

an apex to give the maximum shear for any position of the live load in the panel just in front of that apex. By supposing P successively equal to (w_1), (w_1+w_2), $(w_1+w_2+w_3)\ldots$, we readily deduce, that for those panels where

$R \underline{<} w_1 \frac{l}{b}$ that w_1 *rests at the right apex.*

For those panels where R exceeds $\left(w_1 \frac{l}{b}\right)$ but,

$R \underline{<} (w_1+w_2)\frac{l}{b}$, w_2 *rests at the right apex.*

When R exceeds $(w_1+w_2)\frac{l}{b}$, but,

$R \underline{<} (w_1+w_2+_3)\frac{l}{b}$, w_3 *rests at right apex,*

&c., &c.

11. For convenience we may call the portions of the span commanded by w_1, w_2, w_3, ..., respectively *first field, second field,* &c.

12. In applying the above principles, we first compute the right member of one of the above equalities and find the value of R from the table of loads equal to it; or (if there are none equal), less than, but nearest this value and record the length of load corresponding. Then if the length of load *from the weight commanding the apex, to the wheel load following the last one included in* R, is *equal to* a certain number of panel lengths, the field commanded by the weight extends so far, otherwise the field extends to the nearest apex to the right, where, generally R is less than before. When the supposed uniform load of 2,240 lbs. per foot is on the bridge, values of R can be obtained, exactly equal to the above right members, so that the length of load from the weight commanding the apex to the end of R is taken above, in ascertaining the extent of the field.

13. An example will render this plain. Take the span 200 feet, panel length 20 feet. Then w_1 commands when

$$R \underline{<} w_1 \frac{l}{b} = 75{,}500.$$

The next less value in table is

$$R = (w_1 + \ldots + w_8) = 73{,}390$$

corresponding to a length from w_1 to w_8 of 45.2 feet, hence w_1 commands apices 20′ and 40′ from right abutment. For second field commanded by w_2,

$$R \underline{>} (w_1+w_2)\ 10 = 185{,}500$$

This weight gives a length of R exactly 125 feet. Hence from w_2 to right abutment $= 116.9$; therefore w_2 commands apices 60, 80 and 100 feet from right. w_3 next commands up to

$$R \underline{<} (w_1+w_2+w_3)\ 10 = 295{,}500,$$

or the balance of the span, as we see that R cannot attain to this value from tables for this span.

14. We have hitherto supposed that none of the loads, $w_1, \ldots, w_n$, pass to the left of A, or the apex in front of the one being considered, as rarely happens for usual panel lengths.

Should w_1 pass to left of A, when the loads are shifted a to left, formula (1) must be modified.

Call c the distance from w_1 to A, before the shifting (w_n being at B), then when the loads are shifted a to left, so that w_{n+1} rests at B, the shear as before is increased by

$$R \frac{a}{l},$$

but diminished by

$$\frac{w_1 c + (w_2 + \ldots + w_n)\, a}{b}$$

since there is no increase in the downward effect of w_1 in the shear formula, when it passes A.

We have therefore,

$$S - S' = \frac{1}{l}\left[R - \left\{ w_1 \frac{c}{a} + (w_2 + \ldots + w_n) \right\} \frac{l}{b}\right] \quad a \quad . \quad . \quad (2)$$

15. When the [ ] is +, the shear is increased by the shifting, so that w_{n+1} rests at B, otherwise w_n commands B. In this formula a must not be less than c. In fact if we replace a by the variable x, then during the shifting to the left, till w_1 reaches A, the term $w_1 \frac{c}{x}$ is replaced by w_1. When w_1 passes to the left of A, it remains $w_1 \frac{c}{x}$ up to the limit $x=a$; hence if the [ ] is negative for $x=a$, it will continue so for any value of x less than a; hence when $x=o$, or w_n at B.

16. Next, let us consider the modifying influence of new loads coming on the span. Thus, let an isolated load Q just reach the right abutment when the loads have been shifted a distance x_1 to left.

At that instant by (1), the shear has been altered by

$$\frac{1}{l}\left(\mathrm{R}-\mathrm{P}\frac{l}{b}\right)x_1$$

Now if the loads are shifted y more to left,

$$\mathrm{S}-\mathrm{S}'=\frac{1}{l}\left\{\left(\mathrm{R}-\mathrm{P}\frac{l}{b}\right)(x_1+y)+\mathrm{Q}y\right\}$$

$$=\frac{1}{l}\left[\left(\mathrm{R}-\mathrm{P}\frac{l}{b}\right)x_1+\left(\mathrm{R}-\mathrm{P}\frac{l}{b}+\mathrm{Q}\right)y\right] \quad (3)$$

If (1) is positive, all the more is (3) positive, and both attain their maximum for $x=x_1+y=a$, or w_{n+1} at B.

If the $\square$ in (3) is negative for $y=o$, still if $(\mathrm{R}-\mathrm{P}\frac{l}{b}+\mathrm{Q})$ is positive, a value may possibly be given to y less than $(a-x_1)$ that will cause (3) to become positive, when of course the shear increases with y up to its limit $(a-x_1)$, so that w_{n+1} rests at B. This is the only case where the conclusion derived from (1) is modified, and we shall find in practice that for such apices, the difference in shear due to taking w_n or w_{n+1} at apex is insignificant so that this investigation possesses only a theoretical interest, and need not generally be resorted to in practice, except for small spans.

For the case where the $\square$ in (3) and of course in (1) is negative for $y=a-x_1$ (or $x=a$) the results are the same, *i.e.*, w_n commands at B.

17. If the "new load" coming on is uniform and p pounds per foot, the left reaction is increased, when the loads are shifted x to left by $\frac{p\,x^2}{2l}$, and (1) is replaced by

$$\mathrm{S}-\mathrm{S}'=\frac{1}{l}\left(\mathrm{R}-\mathrm{P}\frac{l}{b}+\frac{px}{2}\right)x \quad . \quad . \quad (4)$$

If the $\square$ is + for $x\leq a$, $(\mathrm{S}-\mathrm{S}')$ increases with x, and is a maximum for $x=a$ or w_{n+1} at B.

If eq. (1) is negative still (4) may be positive, for a certain value of x, so that (1) would require w_n to rest at B, whilst (4) gives correctly w_{n+1} the command of that apex. The difference in shears is only a few hundred pounds though, at the outside, and diminishes as the span increases, being $\frac{1}{4}$ for a 400′ span what it is for a 100′ span.

18. For all cases, whether new loads coming on span are considered or not, we have seen that *for maximum shear, a wheel load is at the apex considered.*

19. Let us ascertain the modifications of the results in the last example when new loads coming on span are considered.

Thus w_1 was found to command the apex 40 feet from the right abutment. When in that position $\mathrm{R}=w_1+\ldots+w_7=66110$. Now, suppose loads shifted 8.1 feet to left; w_2 moves on 7.7 feet and w_3 3.3 feet; the left reaction is then increased by

$$7280\,\frac{7.7+3.3}{200}=400 \text{ lbs.},$$

and the shear by

$$\left(\frac{\mathrm{R}}{l}-\frac{w_1}{b}\right)8.1+400=$$

$$(330-377)8.1+400=20 \text{ pounds},$$

so that strictly the shear is increased 20 pounds by moving loads to left 8.1 feet, and w_2 commands apex 40′ from right. In practice this change is immaterial. The apices in the second and third fields above are unaltered by the consideration of the new loads; thus for w_3 at middle of span, R is 108.1 ft. long and $=166{,}600$, whence by (4),

$$\mathrm{S}-\mathrm{S}'=\tfrac{1}{200}[166{,}600-185{,}500+3192]x,$$

$$\therefore\ \mathrm{S}-\mathrm{S}'<0:$$

whence w_2 remains at central apex for maximum shear.

20. For a solid beam, the whole load P is minus in the shear formula, therefore since the whole system of loads must be shifted to the left a distance a, if at all, since the reaction is steadily increased without any increase in the minus terms, we have:

$$\mathrm{S}-\mathrm{S}'=\frac{1}{l}(\mathrm{R}a+\mathrm{Q}\lambda)-\mathrm{P} \quad . \quad . \quad (5)$$

where Q represents the new load moving on the span and λ its lever arm about the right abutment when the loads have been shifted a distance a to the left. Compare DuBois's *Strains in Framed Structures*, p. 211, where this case is solved.

From (5) we see at once that when $\mathrm{R}a+\mathrm{Q}\lambda>\mathrm{P}l$, the shear is increased by

moving w_{n+1} to B, otherwise w_n remains at B. The application is so obvious that an illustration was not considered necessary.

21. UNIFORM LOAD.—Suppose, in Fig. 3, the weights w_1, w_2.... on span to be replaced by a uniform load extending a distance z to the left of B, to find the value of z, in order that the shear be a maximum. Call c=distance from B to right abutment.

Now the shear is *increased* on moving the uniform load to left so long as the reaction at left abutment of a small part of the load immediately in front is greater than the amount of this load supported at A, and *diminished* beyond the point where the reaction and part held at A are equal. At this last point, which marks the extremity of the load for maximum shear, we have—

$$\frac{z}{b}=\frac{c+z}{l}$$

$$\therefore\ z=\frac{c}{\mathrm{N}-1}$$

$$\therefore\ c+z=\frac{\mathrm{N}c}{\mathrm{N}-1}=\frac{c}{b}\,\frac{\mathrm{N}b}{\mathrm{N}-1}$$

where N = number of panels in span $\therefore Nb=l$, therefore, *length of load is*

$$c+z=\frac{c}{b}\cdot\frac{l}{\mathrm{N}-1}.$$

Hence, divide the span into N−1 equal parts, then as c takes successively the the values b, $2b$, $3b$ ..., the load reaches from the right abutment to the 1st, 2d, ... divisions of the span for maximum shear. This result is obtained in another way in *Wood's Bridges*, page 126, edition of 1876. For a solid beam, the uniform load reaches to the point considered, as any load in front diminishes the shear. In fact, we see that as N increases indefinitely, that z, in a formula above, tends towards zero as its limit.

The method of apex loads gives a sufficiently near approximation in practice, and it looks like too great a refinement to specify any other, especially as the approximation gives a slight excess, and the assumed conditions of loading are but rarely realized in practice. In fact, for highway bridges, the uniform moving load, assumed in the calculations, is very rarely experienced, so that a higher unit strain is permissible than for railroad bridges, where the computed strains are frequently realized.

22. COMPUTATION OF STRESSES IN CHORD AND WEB MEMBERS.

After the position of the live load, giving maximum stresses is found, the computation for the live load *on one truss*, assumed in article 2, is very much facilitated by the use of the following table, giving the moment, about each

w	M.	w.	M.
w_1	0,000,000	w_{10}	2,585,858
w_2	61,155	w_{11}	3,300,440
w_3	166,890	w_{12}	3,865,994
w_4	299,865	w_{13}	4,361,984
w_5	482,340	w_{14}	4,907,474
w_6	848,345	w_{15}	5,846,236
w_7	1,130,729	w_{16}	6,515,836
w_8	1,507,556	w_{17}	7,352,482
w_9	1,859,828	w_{18}	8,091,970
		p	8,737,330

weight in turn, of the live loads to the left of that weight. In making the table, each successive moment was derived from the preceding, and checked independently at intervals and at the end. Thus to find the moment about p, the front of the uniform load from the preceding moment. We know $(w_1+w_2+\ +\ ..+w_{18})=161{,}340$ pounds, and that it is 4 feet from w_{18} to p.

$\therefore\ \mathrm{M}_p=8{,}091{,}970+161{,}340\times4=8{,}737{,}330.$

By similar principles we find, the moment about a point x feet to right of p,

$$=8{,}737{,}330+161{,}340x+560x^2.$$

For any other live load than that assumed, and of course specifications vary in this regard, tables can be made out like the preceding, and the results written on a diagram of the loads, to scale. If the apex points of a given truss are laid off to the same scale, the diagram of loads can be placed alongside in position, and the proper distances, weights, moments, etc., taken off at a glance.

For the left *reaction*, we divide the moment about the right abutment by the length of span. This moment is found from the formula just given, when any of the uniform load is on the span, x rep-

resenting, the length of this uniform load on the span. When none of the uniform load is on the span, if a wheel load is at the right abutment, the preceding table gives the moment M at once. If the right abutment is y feet from the nearest wheel load w_n, the moment about the right abutment =

$$M_n + (w_1 + w_2 + \ldots + w_n)y.$$

Since the moment about any wheel of the load to the left of it is given directly by the table, we quickly find the moment of the live load about any apex of the truss required for the computation of chords.

For shear, the reaction is found in the same way. The load supported at the apex A (fig. 3) is found by dividing the proper moment in the table by a panel length. Subtracting this from the reaction, we have the shear.

All of this detail has been entered into in order that the article may prove of value in practice, particularly to those who rarely have occasion to make such computations.

THE TRUE GRAVIMETRIC CONSTANT.

By JACOB M. CLARK.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

PROPOSITION.

THE space through which a body at mean latitude near the surface of the earth, *in vacuo*, descends by virtue of the accelerating force of gravity, in $\frac{1}{1,000}$ of an hour, is precisely 2,500 geometric inches =100 geometric cubits = the side of a geometric square acre, so near as we know the constant.

[The geometric inch is taken at $\frac{1}{1,000,000,000}$ part of twice the earth's polar axis, and is $1\frac{1}{1,000}$ English inches, very nearly.]

The decimal correlation shown in the following table is contained in the existing expressions, founded on Newton's law; but is concealed by the use of sexagesimal notation, and of a 12-inch foot, founded on an inch slightly out of correlation with the geometric constant for latitude 45°. It does not appear except by dividing the circle, both for time and angular measure, *geometrically* (*i. e.*, by 24×10^n), and taking the unit of linear measure at a decimal of the earth's semi-polar axis.

If the English inch is used, the same expressions appear very nearly—we may say exactly—for latitude 30°. But for scientific use, the constant for latitude 45° is vastly to be preferred, both by reasons of its exact decimal relation to the semi-polar axis and of the superior simplicity with which the correction for latitude is applied.

Time in thousandths of an hour.	Acquired velocity, cubits.	Squares of the time.	Total descent, cubits.	Ratio of spaces in intervals.	Descent in separate of time, cubits.
1	200	1	100	1	100
2	400	4	400	3	300
3	600	9	900	5	500
4	800	16	1,600	7	700
5	1,000	25	2,500	9	900
6	1,200	36	3,600	11	1,100
7	1,400	49	4,900	13	1,300
8	1,600	64	6,400	15	1,500
9	1,800	81	8,100	17	1,700
10	2,000	100	10,000	19	1,900

That is	Cubits.	Acre, Sides.
In $\frac{1}{10,000}$ of an hour, the total descent	= 1	= .01
in $\frac{1}{1,000}$ " " "	= 100	= 1
in $\frac{1}{100}$ " " "	= 10,000	= 100
in $\frac{1}{10}$ " " "	= 1,000,000	= 10,000
and in one hour equal to 10 times the earth's semi-axis	= 100,000,000	= 1,000,000

THE SCIENTIFIC STUDY OF NAVAL ARCHITECTURE.*

By FRANCIS ELGAR, John Elder Professor of Naval Architecture.

From the "Nautical Magazine."

We have met together to-day upon no ordinary occasion. The institution of the John Elder Chair of Naval Architecture in this University is, in more senses than one, an unique and important event. No British University has ever before included in its curriculum anything like a complete treatment of the science of naval architecture; and this is also the first time that mercantile naval architects and shipbuilders have had an opportunity of commencing a course of scientific training which is intended to be specially and fully adapted to their requirements. Unlike nearly all the professors whom I have now the honor to call my colleagues, I have no predecessors, the records or traditions of whose life and work I can appeal to for encouragement, or inspiration, or as a pattern for imitation.

Although that is the case, however, so far as this Chair is concerned, many of the subjects which are to be taught from it have already had a far abler exponent in this University than I can ever aspire to become. The late Professor Macquorn Rankine was for many years one of the greatest authorities upon the sciences related to naval architecture and marine engineering. His immense ability and energy enabled him to achieve results in these departments of knowledge which would have made him famous had he attempted nothing else. But when we remember that what he thus did constituted but a small portion of his life-work, and was only connected with a few out of the many subjects comprised under the head of "Civil Engineering and Applied Mechanics," with all of which he dealt, we may truly marvel at what he accomplished for naval architecture. I cannot speak from personal experience of Professor Rankine's teaching in this University; but I am grateful for the opportunity of paying an humble tribute to his memory, in this place where he was so well known and so highly esteemed, by referring to what he did elsewhere.

Professor Rankine was a lecturer at the Royal School of Naval Architecture and Marine Engineering, at which I was a student; and he was one of the ablest members of, and most regular attendants at, the Institution of Naval Architects in London. There were very able men in London in those days at both the institutions I have named; and great advances were being made in the science of naval architecture, and in its application to the practical requirements of shipbuilders. Problems which had previously baffled all attempts at solution, even when made by some of the most eminent mathematicians of this and the previous century, were at length yielding to the genius and methods of modern investigators. The way was led by the late Mr. William Froude, with a paper upon "The Rolling of Ships," read before the Institution of Naval Architects in 1861.

Mr. Froude brought forward in this paper what, in his own words, "assumes to be a tolerably complete theoretical elucidation of a difficult and intricate subject, which has hitherto been treated as if unapproachable by the methods of regular investigation." I hardly need pause to say that Mr. Froude accom-

* Inaugural address, delivered in the University of Glasgow, November 11, 1884.

plished his self-imposed and formidable task with marvelous success. He thereby initiated a new era in the history of naval architecture, and one in which Professor Rankine was among the most prominent and distinguished figures. Professor Rankine helped more than any one to confirm and extend Mr. Froude's theories of wave motion and of the rolling of ships; and he also devoted himself with great ability and thoroughness to an investigation of the laws which govern the resistance of ships, and to the whole question of marine propulsion.

Professor Rankine impressed the students in London with the most profound respect and admiration for his great powers, and for the original and masterly way in which he dealt with everything he touched; while he possessed the rare gift of rousing their energies by his personal enthusiasm and charm of manner. No man with whom the students of naval architecture then came in contact had so powerful and elevating an influence upon their minds as Professor Rankine; and there is no one to whom they owe more, either for what he directly taught or for the mental stimulus they received by contact with him. It is a peculiar gratification to me, remembering what Professor Rankine was to the students of naval architecture of my time, to be able to bear testimony here to the high regard we all had for him, and to the high esteem in which we hold his memory.

It has formerly been thought necessary by some, who have been called upon to advocate or initiate courses of scientific teaching of naval architecture, to defend their position argumentatively, and even apologetically. I do not feel, however, that the circumstances of the present case require from me anything of the nature of an *apologia*. It will, perhaps, be more appropriate to the occasion, and more instructive to you, if I pass on at once to attempt to show how and when the recent irresistible demand for improved scientific knowledge, in virtue of which we are here to-day, arose in the Mercantile Marine; and shipbuilders became convinced, not merely of the desirability, but of the absolute necessity, of long and severe courses of preparatory scientific study, such as their forefathers, to all appearances, got on fairly well without. Perhaps a few discursive remarks upon these and other topics of general interest to the shipbuilding profession may also prove more acceptable to you to-day than a dissertation upon purely technical or abstract subjects.

The demand for scientific training in naval architecture is a comparatively modern one so far as the Mercantile Marine is concerned. It has long existed, however, in connection with the requirements of the Royal Navy. Even so long ago as Sir Walter Raleigh's day the builders of ships of war were reproached for want of technical knowledge, and for the errors and failures consequent upon it. That celebrated author, in his "Discourse on the Royal Navy and Sea Service," calls attention to the injurious effect upon a vessel's sailing qualities which is produced by over-immersion. He says "that the ship-wrights be not deceived herein (as for the most part they have ever been) they must be sure that the ship sinck no deeper into the water than they promise, for otherwise the bow and quarter will utterly spoile her sayling." Such complaints did not apply to the war-ships of this country only, for we find one of the earliest French writers upon naval architecture, Père l'Hoste, saying in 1697 of the ships of the French Navy, "It cannot be denied that the art of constructing ships, which is so necessary to the State, is the least perfect of all the arts. . . . Chance has so much to do with construction that the ships which are built with the greatest care, are commonly the worst, and those which are built carelessly are sometimes the best. Thus the largest ships are often the most defective, and more good ships are seen amongst the merchantmen than in the Royal Navy."

It was in the construction of war-ships that the greatest difficulties formerly arose, and that the need of scientific knowledge and improved methods of design first became manifest. Merchantmen were smaller in size, and much more simple and uniform in type and proportions, than war-ships, until quite recent times. If we look at the Mercantile Marine of fifty years ago we find that there were then about 750 merchant vessels built in each year. Out of these 750 vessels, only about 40 were above 300 tons, old builders' measurement, and no more

than 10 exceeded 500 tons. The whole number of ships composing the British Mercantile Marine in 1830 amounted to 19,110, of which but 168 were above 500 tons measurement. Thus more than 99 per cent. of our merchant vessels were at that time of less than 500 tons measurement. Very few were ever built of over 130 feet in length.* Mr. James Laing, of Sunderland, has been good enough to furnish me with particulars of a vessel which was built by his father, for his own use as a shipowner, in 1815. She is interesting as having been one of the first of the free traders to Calcutta after the breaking up in that year of the monopoly so long enjoyed by the East India Company. The length of this vessel was 109 feet 9 inches, breadth 29 feet 7 inches, and depth 20 feet 6 inches; the tonnage being 414. This is a typical illustration of a fine merchantman of that period, such as was employed upon the longest sea voyages. She was named the *Caledonia*, and successfully performed the voyage to India and back in about 10½ months, which was considered good work in those days.

It is clear that mercantile shipbuilders could not have been much troubled up to that time by difficult or novel problems in naval construction. They were simply occupied with the building of wooden craft of comparatively uniform types and proportions, and of so small a size that not one per cent. of the whole number exceeded 500 tons in measurement. The case was quite different, and had long been different, with the constructors of war ships. From the days of Henry VII. until now, successive Sovereigns and Governments of this country have striven in a more or less degree, first to rival, and afterwards to surpass other naval Powers in the strength and excellence of their ships of war. The efforts made at various times with this view have not always been wise nor successful. Large ships which could carry numerous guns, and carry them high above the water level with safety, and without serious detriment to sailing qualities, constituted the great and ever-growing, but very difficult, requirement of the naval authorities; and formed the problem which was continually proving a stumbling-block to successive generations of naval constructors. Henry VII. built several large vessels, one of which measured, according to various accounts, from 1,000 to 1,500 tons. In the fleet that sailed to meet the Spanish Armada there were two ships which exceeded 1,000 tons in burthen. James I. built a vessel which measured 1,400 tons; while the *Royal Sovereign*, built by Charles I. in 1637, was said to be "just so many tons in burthen as there have been years since our blessed Saviour's Incarnation, namely, 1637, and not one under or over."

Coming to more recent times we find that, at the commencement of the present century, British first-rate ships of the line had increased in size to 2,000, and even to 2,500, tons measurement. These vessels were heavily laden with top hamper, in the shape of three tiers of decks with numerous guns upon them. They frequently proved to be over-draught and unstable upon trial, and deficient in weatherly qualities at sea. No questions ever came before mercantile shipbuilders in that day of such magnitude, difficulty, or complexity, as those which war-ship constructors had to deal with. Mercantile shipbuilders produced small craft of a class whose qualities had been thoroughly tested by experience and were well understood by the owners and masters who had to work them. War-ship constructors, on the other hand, were the designers of comparatively huge vessels, whose type and loading introduced elements of special difficulty and danger.

Instability required to be carefully guarded against in war-ships of the description referred to, but our naval constructors did not then understand the principles upon which stability depends. Ships frequently proved so unstable when completed that all the remedies which could be devised, such as carrying extra ballast and doubling the planking at the water line, had to be applied. It is true that mathematical treatises upon the stability of ships had long been published in France and elsewhere; but they had been of no effect in improving the practice of naval architecture in this country. Even in France, where Daniel Bernouilli obtained a prize offered by

* Annals of Lloyd's Register, 1884.

the *Société Royale des Sciences* in the year 1757, for a *mémoire* in which the statical conditions of stability are clearly stated, high authorities upon naval architecture were unable, thirty years after, to account for a deficiency of stability in three ships that were remarkably crank.* In this country, where naval designers were much more deficient in scientific knowledge than in France, and where war ships were notoriously inferior to those of the French, mistakes were more general, and were frequently more fundamental and serious.

Mr. Wilson, a member of the first School of Naval Architecture, gives † an instructive and interesting account of the cutting down of a 64-gun two-decked ship of the line to a frigate of 38 guns, in the year 1794. He says that "so culpably ignorant were the English constructors that this operation, so well calculated, when properly conducted, to produce a good ship, was a complete failure. Seven feet of the upper part of the topsides, together with a deck and guns, weighing about 160 tons, were removed, by which her stability was greatly increased; but by a complete absurdity the sails were reduced one-sixth in area. In her first voyage the rolling was so excessive that she sprang several sets of topmasts. To mitigate this evil, in 1795 her masts and yards were increased to their original size; but as there was no decrease of ballast, she was still a very uneasy ship, and as a necessary result, her wear and tear were excessive.

Other sixty-fours were cut down, masted and ballasted in exactly the same manner, and, it need scarcely be added, experienced similar misfortunes; and although they were improved by enlarging their masts and yards, they were still bad ships. Had their transformations been scientifically conducted, a class of frigates would have been continued in the Navy, capable from their size, of coping with the large American frigates; and thus the disasters we experienced in the late war, from the superior force of that nation, would, without doubt, have been, not merely avoided, but turned into occurrences of a quite opposite character.

I may here say in passing that one of the chief reasons why shipbuilders, shipmasters and others who require to understand the principles upon which a ship's qualities depend, often remain for long periods in ignorance of the published information upon the subject is perhaps to be found in the fact that the style of treatment commonly adopted in works upon naval architecture is one which presupposes and requires advanced mathematical and highly technical knowledge in the readers. The celebrated mathematician, Euler, said in 1773, in the dedication of his work entitled, "Théorie Complette de la Construction et de la Manœuvre des Vaisseaux," that "although forty years have elapsed since mathematicians have labored with some success, yet their discoveries are so much enveloped in profound calculations that mariners have scarce been able to derive any benefit from them." This reproach still attaches, I fear, to many writings upon naval architecture; and in removing the cause for it more may probably be done for the benefit and enlightment of students than in any other way. The great want of the time, in this department of science, is elementary explanations of principles and processes stated in clear and precise language, but freed as much as possible from advanced mathematical methods and terms, and from perplexing technicalities.

The difficulties of war-ship construction had become so overwhelming at the end of the last century owing to the above-named, and other causes that radical measures for remedying them could no longer be delayed. No such difficulties had arisen, however, be it observed, even at a much later date, in connection with the ships of the Mercantile Marine. Several attempts were made to improve the existing state of things in the Royal Navy, and to promote the spread of scientific knowledge of naval architecture in this country. These resulted in the establishment of the first School of Naval Architecture by the Admiralty, at Portsmouth, in the year 1811. Dr. Inman, the principal of the school, said, in an official document, printed by order of the House of Commons in 1833, that at that period "scarcely a single individual in this country knew correctly even the first element of the displacement of one

* L'art de la Marine. M. Romme.

† Papers upon Naval Architecture and other subjects connected with Naval Science. 1827–1833.

of our numerous ships, either light or load." So far as the Mercantile Marine was concerned this may not then have been of much practical importance, but in the Navy the case was very different.

The first School of Naval Architecture remained in operation during more than twenty years, and trained about forty students. The second School of Naval Architecture was founded at Portsmouth by the Admiralty in 1848, with Dr. Woolley as the Principal. This school had only a brief existence, and was closed in the course of a very few years. The third School of Naval Architecture, which also included marine engineering, was opened at South Kensington in 1864, and is now united with the Royal Naval College at Greenwich. The whole of these schools were instituted and carried on for the special purpose of training up war-ship designers and calculators for the work of the Royal Navy; and many students, highly trained in mathematics, and in the special work of war-ship construction, were educated in them. The members of the first School of Naval Architecture were for a long time the victims of professional jealousies and prejudices, and had to contend against the strong opposition of the old class of officers at the Admiralty and in the Dockyards. They were kept in subordinate positions until late in life; and it became the custom for the First Lord of the Admiralty to state annually, from his place in Parliament, that these "young men" (men between 40 and 50 years of age) "though gentlemen, and men of education, yet want experience, and therefore cannot be promoted." The members of the later schools of naval architecture have not had similar difficulties to contend with to anything like the same extent. Some now occupy the highest positions in Her Majesty's service, and have done much to advance the science of naval architecture, and especially to bring about great and long needed improvements in war-ship design.

The demand for improved scientific knowledge of naval architecture has thus existed in connection with the Royal Navy from a very early period, and was long of a most pressing character. The first attempts to supply it had already been made by the establishment of the first School of Naval Architecture, long before the Mercantile Marine was even affected. No difficulties approaching in any degree to those met with by war-ship designers arose to vex the souls of mercantile shipbuilders till after the modern age of steam shipping was entered upon, and till new types of vessels of enormous sizes and novel proportions were designed, in the construction of which previous shipbuilding experience was an unsafe or an insufficient guide.

It is interesting in this connection to observe that the Admiralty practice and that of the Mercantile Marine appear to have been very similar, even down to the present time, in dealing with small craft of old-fashioned types and proportions, which existed in sufficient numbers to enable the qualities of any one vessel to be inferred from those which others were known by experience to possess. At the court martial held to enquire into the cause of the capsizing of the small wooden frigate *Eurydice* in 1878, Mr. Barnaby, the Director of Naval Construction, said that she was inclined under his direction on the 11th May, 1877, not for the purpose of discovering whether she was a stable ship, but as a matter of scientific interest, because, so far as he knew, no sailing frigate or larger sailing ship had ever been inclined in the history of the Royal Navy. Mr. Barnaby explained that, "The reason for this must be the same as that which rules the present practice in the merchant Navy. There are about 5,000,000 tons of registered sailing ships in Great Britain, and it is not the practice of any owners to incline their ships." At the inquiry into the loss of H.M.S. *Atalanta*, another small wooden frigate, two years later, in 1880, Mr. Barnaby said: "The ship was never inclined so as to have the center of gravity ascertained. Her stability was known only in the sense that she was like, or nearly like, other ships whose behavior is well known." These statements show that the practice of the Admiralty and that of the Mercantile Marine have been very similar throughout in regard to the construction of ships of ordinary type, which do not appear to contain elements of special difficulty or danger. It was the development of peculiar types of ships,

possessing uncommon or abnormal features, which first made elaborate scientific treatment necessary in both cases, and to which it has latterly been applied. The exceptional difficulties caused by such development arose much earlier in the Royal Navy than in the Mercantile Marine, and therefore had to be dealt with at an earlier period, and at a time when the science of naval architecture was not nearly so advanced as it now is.

Fifty years ago the use of iron for ship construction and the employment of steam propulsion had only been attempted in a few vessels that were employed in coasting or river trades. As regards iron, few persons imagined that it was the material of the future for shipbuilding purposes. Although its use had, for some time, been advocated by a few able and far-seeing men, and some small craft had been constructed of it, the public and the great body of shipbuilders refused, in 1830, to believe that the wooden walls of old England were to be supplanted by a material that would naturally sink. "Who ever heard," it was derisively asked, "of iron floating?" The chief constructor of one of our Royal Naval Dockyards said to Mr. Scott Russell, with a feeling so strong, and with indignation so natural, that the latter never forgot it, "Don't talk to me about iron ships; it is contrary to nature." Steam propulsion was making progress, but it was not yet considered suitable for over-sea trades. Mr. David Napier had made engines of 200 horse-power; and lines of steamers were plying between Liverpool and the Clyde, and between London and Edinburgh. It was not thought possible, however, to make long voyages by means of steam propulsion. Men of high scientific reputation and position believed, in 1835, that in the then state of the marine engine the project of making a voyage by steam alone directly from New York to Liverpool was perfectly chimerical, and that persons might as well talk of "making a voyage from New York or Liverpool to the moon."

The shipbuilders of the old school held back as long as possible from taking the leap in the dark which was involved by commencing the production of iron steamers. The way was at first led, not by the great shipbuilders of the day, but by eminent engineers, such as Napier, Fairbairn, Brunel, Scott Russell, and others, who investigated and solved the leading structural and other problems involved in this great revolution in shipbuilding. It was at the request of Mr. Brunel, and for his guidance in designing the *Great Eastern*, that his friend Mr. William Froude commenced in 1856 his investigations into the laws of motion of a ship among waves. It was not till after mercantile shipbuilders began to build vessels of far greater sizes than those prevalent fifty years ago that the present demand for improved scientific knowledge began to be felt by them. The fact is that the modern changes in shipbuilding practice which followed upon the substitution of iron for wood as the material of construction, and the use of steam propulsion, not only increased the difficulties of manufacture, but they have gradually brought about a change in the shipbuilder's position with reference to his work, and in the nature of his responsibility for it.

Formerly a shipbuilder was merely the builder of a ship, in reality as well as in name. No better mechanics ever existed, nor men more skilled in the geometry and other practical sciences which bore directly upon their work, than many of the shipbuilders of the past. They were perfect masters of what they undertook to do, and possessed a vast amount of special knowledge and ingenuity. Many of the methods by which irregularly-shaped pieces of timber were prepared to their requisite forms involved geometrical processes of an extremely complicated and difficult kind. Some of the problems that were dealt with in practice by the old school of shipwrights would now puzzle many advanced students of descriptive and solid geometry. Shipbuilding was then a highly-developed mechanical art, much of the knowledge of which is not now required, and is, consequently, fast dying out.

The business of the shipbuilder used to be limited to the production of ships of the dimensions and description required by owners; and to building them of good sound timber, well fitted and fastened together. No elaborate calculations were requisite for determining whether one of these vessels would stand up when light, or be stable when laden with cer-

tain cargoes; nor were any but the roughest approximate methods necessary even for estimating the displacement or carrying capacity. Owners and masters, as well as the builders, had ample materials, derived from experience and from the observation of many similar vessels, by which to form their own judgments upon such points. Usually these ships would not stand up, when fully rigged and light, without ballast, and judging from the proportions given to them they must also have required ballast when laden with cargoes which were not composed of heavy dead-weight. In most cases iron kentledge was provided for them.

Few of these vessels would shift without ballast, except such as were of the old collier type, and were specially built for the coal trade. It is notorious, however, that the chief reason why so many of the ships of that day were crank, is to be found in the operation of the old tonnage laws, which took breadth into account in estimating tonnage, and ignored depth. Ships were built of great relative depths in proportion to their breadths, and initial stability was deliberately sacrificed in order to reduce the tonnage measurement. The instability of these ships was of such a character, however, owing to their form and proportions, that it could be dealt with and corrected in a practical manner, by means of the trained judgment and experience of the masters and stevedores. Any deficiency of stability was fully indicated by initial tenderness, and the curing of it was simply a question of putting ballast into the bottom. The sail-carrying power at sea usually furnished a good test of stability; and the experience thus gained was practically utilized in loading and ballasting vessels of all sizes and classes. It is important to observe that the instability which these vessels possessed was not of that dangerous and treacherous quality which exists in many modern steamers, and which renders them liable to capsize without previously giving obvious indications by which those on board may be sufficiently trusted to judge of their danger.

The mercantile shipbuilder of the present day has problems of a very different and much more complex and difficult character to deal with than his predecessors, and that is why the necessity for improved scientific knowledge is now so strongly felt. Many of the details of the mechanical work of construction are really simpler, and do not call for the exercise of the builder's personal skill and ingenuity, so far as the hull of the ship is concerned, to such an extent as formerly. What is now required of him is to predict with great accuracy, the weights of complicated iron and steel structures, of various types and sizes, with all their intricate fittings and machinery; the weight of cargo that such structures will carry at sea; the stability they will possess in different conditions of loading, and the treatment necessary to ensure a safe amount of stability being preserved upon all occasions; the amount of steam-power and the rate of coal consumption required to maintain given speeds at sea; and very frequently, the strength that is possessed by the hull to resist the straining action of waves.

Problems like these may now be put before a shipbuilder any day for solution, or, if he neglects to consider them for himself, when constructing certain types of vessels, he may afterwards be held to blame in the event of some unforeseen failure or casualty occurring. Disasters to ships that were once unhesitatingly, and even reverently, attributed to the "act of God" are now seen to be controllable, in many instances, by man, if such knowledge and foresight as he has the power of acquiring, be applied to the purpose. We now, perhaps, suffer occasionally from a reaction towards the opposite extreme, and too much may sometimes be expected of shipbuilders and shipowners in the way of preventing disasters at sea. It does not always appear to be sufficiently borne in mind that, whatever advances may have been made in the application of scientific knowledge and of practical mechanical skill to the construction of ships, men have not yet acquired the power of absolutely dominating all those vast and indefinable forces which nature frequently brings into play upon the ocean.

It is true that shipbuilders still build many ships, as formerly, to detailed specifications, prepared and furnished by the owners, but, even then, they appear liable to responsibilities which, though at pres-

ent very unsettled and indeterminate in their scope and character, and often quite unintended or unexpected by anyone, are not the less heavy and real. The time has arrived when it is evident that naval architects and shipbuilders require to possess a thorough knowledge of those natural laws upon which the qualities of ships and their safety at sea depend. Such knowledge is necessary, not only to prevent error or disaster in extreme cases, but for the more ordinary and commonplace, though not unimportant, purpose of enabling the requirements of a specification, or the stringent guarantees that are often contained in contracts, to be fulfilled in the simplest and most economical manner that is consistent with the stipulated degree of efficiency. It would be impossible for me to enumerate at the present time all the questions in which sound scientific principles are of importance to the naval architect of the present day, and with which he should endeavor to become acquainted. It is a knowledge of principles rather than of results that he should mainly aim at acquiring; because his information requires to be of that well-grounded, broad, and general character which is readily and directly applicable to novel and everchanging circumstances; and which may be acted upon with certainty and promptitude in difficult cases. Dr. Woolley stated this with great force and clearness in a paper read before the Institution of Naval Architects in 1864; and though he was then specially addressing the constructors of war ships, I cannot find any words more applicable to the present requirements of mercantile naval architecture. He said that the only way in which superiority in shipbuilding can be acquired is, "by possessing a class of shipbuilders trained in mathematical science with the powers of their minds invigorated and strengthened by a profound and severe course of study, able to deal with questions to which altered conditions are continually giving rise, not by trial and error—which is most frequently but another name for failure—not with the hesitating and trembling hand of the superficial sciolist—but with the firm grasp and bold readiness of the man profoundly skilled in the scientific principles of all kinds which may be made available to the art of naval construction, who feels himself thoroughly at home in them, and has acquired such power as to enable him to apply his principles readily and exactly, without fear of failure or of overlooking one principle while anxious to give effect to another."

It has sometimes been asked why, if the necessity for improved scientific knowledge has really been felt in the Mercantile Marine, students have not availed themselves of the educational facilities held out by the late Royal School of Naval Architecture, and by the Royal Naval College at Greenwich? The answer to this question is, to my mind, conclusive, and is only one which furnishes just cause for discouragement to ourselves. The Admiralty Schools of Naval Architecture, and the present Royal Naval College, were organized for the express purpose of supplying the special requirements of the Admiralty service. High mathematical attainments have been expected of all students before entering the college; and the training given to those who have entered has been of an advanced mathematical, and, so far as naval architecture is concerned, of too restricted and special a character for the practical purposes of non-Admiralty students. The instructors and lecturers in naval architecture have, without exception, been able and accomplished naval architects, but they have been specialists in Admiralty war-ship design. This, in my opinion, is alone sufficient to account for much of the want of confidence that has been shown by private shipbuilders in the suitability, for their purposes, of the training offered by the naval College.

The reason why the Admiralty Schools have, in the language of official authorities, been "hopeless failures," so far as the Mercantile Marine is concerned, is because they were too exclusively naval in their character and work, and because no adequate attempts were made to adapt them to the requirements of non-Admiralty students. The differences between the processes that are adopted in the Royal Navy and in the Mercantile Marine in the designing of ships are radical, and can only be properly appreciated by those who happen to be intimately acquainted with both. In the Mercantile Marine economy of time and labor is the chief aim of a designer; and short meth-

ods of calculation or of temporary approximation, which are but little appreciated in the Admiralty service, are employed for the purpose of enabling the work of construction to be quickly commenced and rapidly proceeded with. Economy of time and of cost of production, and how to secure these advantages, are among the chief subjects which mercantile naval architects require to study, and upon the practice of which their success mainly depends. Long periods are frequently occupied in investigating and arranging the details of war-ship design which cannot be obtained in the Mercantile Marine, and which, if insisted upon, would prove an effectual bar to progress in business.

If we consider the practical work of the shipyard, an accurate and full knowledge of which is invaluable to the naval architect, it will be seen how unsuitable is mere Admiralty teaching to the requirements of the Mercantile Marine by a comparison of the costs of labor in the two cases. The work upon the structural iron or steel portions of the hull of a vessel, which in the ships of the Navy often costs, according to the best information I can obtain, £20 per ton of weight, is carried out upon so much more economical and efficient a system in the Mercantile Marine, both as regards the time and labor expended, that in vessels which are at least equal in strength and durability to those of the Royal Navy, as is proved by the work they do, the cost of labor often amounts to no more than £5 per ton of weight. The time element is an equally important factor in the two classes of work, but this is a point which I cannot now pause to consider. The figures for cost of labor that I have given relate to vessels of as similar construction as possible, with water-tight double bottoms. The difference in cost of production is largely attributable to tedious and costly systems of work that are still cherished in the Royal Navy, but which have been long obsolete in the Mercantile Marine, and been supplanted by improved methods.

It is the two circumstances of the growing necessity felt by mercantile shipbuilders for scientific training in naval architecture, and the failure of the Royal Naval College to furnish such training as they require that have mainly led to the foundation of the John Elder Chair of Naval Architecture. And now the question arises of the method we are going to adopt, and of the kind of training we shall endeavor to give here. This will depend greatly upon the intelligence and energy of the students, and upon the amount of mathematical and general scientific knowledge with which they may be furnished when they come here. The course will be adapted, as far as possible, to their state of knowledge and to their practical necessities. But I cannot undertake to describe its scope in detail, nor to define its limits with precision, without some previous experience of the students. I have not come here with hard-and-fast ideas, nor with a cut-and-dried programme. Had I done so, our progress might thereby have been hampered or wrongly directed—it could hardly have been facilitated. I shall endeavor to help the students, to the best of my ability, to acquire a sound and scientific basis for such knowledge of shipbuilding and engineering as they may already possess—and the more they have the better—and to go forward to a complete study of those scientific principles upon the knowledge of which their success in life will greatly depend. At the same time, while insisting in the most unqualified manner upon the absolute necessity for scientific study, I must warn them against supposing that mere attendance at these classes during one, two, or any number of sessions is going to enable a student to become a competent naval architect or engineer. All that can be given here are intellectual tools with which to work with greatly-increased ease and precision in the practical operations of ship design and construction. Theoretical principles, and the manner in which they can be utilized with advantage in practice, will be taught; but it requires very much more to make a man a naval architect than knowledge that may be acquired within an University, however clever or hard-working a student he may be.

As an example of the training best adapted for producing good naval architects or engineers, and as a pattern which all students of these classes may study, and strive to copy with advantage, I cannot do better than refer to the great engineer after whom this Chair has been

named. Mr. John Elder always displayed great talent and application in the study of mathematics, which he diligently pursued in the High School of Glasgow; but he was prevented by a naturally delicate constitution from receiving any University education, except such as was obtained by attendance at the class of Civil Engineering in the old college. He studied privately, however, with great ardor, and acquired a large and varied amount of scientific knowledge, which was also complete and exact, and free from the defects in thoroughness and accuracy which so often beset self-taught scholars. John Elder served an apprenticeship of five years as an engineer in the works of Mr. Robert Napier, working in the pattern-shop, foundry and drawing-office. He afterwards worked as a pattern-maker at Bolton-le-Moors, and as a draughtsman at the great Grimsby Docks. His next situation was that of chief draughtsman under Mr. Napier, which he left three years afterwards to become a partner in the firm of Messrs. Randolph, Elder & Co.

The point to which I now wish particularly to draw your attention is the long and arduous practical training that Mr. Elder went through. It was this combined with his complete scientific knowledge and undoubted natural genius, which enabled him to achieve his great successes in after-life. The highest scientific knowledge attainable is of little use to the naval architect unless it exists in combination with good judgment and practical mechanical skill. Mr. Elder owed both his professional and commercial success to a rare combination of qualities. Prof. Rankine says in the memoir he wrote of him that the different qualifications possessed by Mr. John Elder "are so seldom found united in one man, that the tendency of popular opinion is to regard them as incompatible, and to look especially upon the knowledge, skill, and enterprise which lead an engineer to adopt new or unusual improvements in practice, as being fraught with danger to his success in business, and so no doubt they are, unless regulated by commercial sagacity."

There is, unfortunately, too great a tendency sometimes displayed by enthusiasts in the cause of technical education to elevate mathematical and scientific training above its true position, high as is that to which it is legitimately entitled, and to rely too exclusively upon the results of such training for guidance and power in the performance of large and intricate mechanical operations. It is a *sine qua non* for the modern Naval Architect, although, at the same time, it is by no means sufficient for all his numerous and varied requirements. It is even of little real practical use, unless there underlies it an intimate personal acquaintance with the mechanical operations of the shipyard and engine works, and with the properties and capabilities of the materials there dealt with. Together with this, there must likewise be the faculty, which is more essential than any, and which may be highly cultivated by all open, liberal, and intelligent minds.

"Good sense, which only is the gift of heaven,
And though no science, fairly worth the seven."

I shall not detain you any longer upon the present occasion. We shall commence our regular course of study to-morrow. It will be one which will be adapted, so far as I know how, to your practical needs, and to your present state of knowledge. I hope that the noble profession of naval architecture may one day reckon some of my present students among its chief ornaments; and that the Chair which bears the great and honored name of John Elder may be helpful in training up naval architects and marine engineers to rival him in all that is worthy, good and great.

In a paper on the influence of punching on mild steel, published in the *St. Petersburg Journal* and "Proc. Inst. C. E.," M. W. Beck-Gerard says:—"That although a search for incipient cracks proved fruitless, he has, he believes, for the first time, observed certain markings on the polished surfaces of the plates around the cold punched holes. Visible to the naked eye, and surrounding the holes, were bunches of lines starting tangentially to the holes, and curving slightly toward them. These lines branch out in opposite directions and intersect with some degree of regularity. They do not appear in the vicinity of drilled holes, but are distinct in cold punched holes reamed out. In forged iron they did not appear, although they were most distinct in the softest steel, and vanished when the metal reached the hardness due to 0.6 per cent. of carbon. An increase of thickness in the plate caused a corresponding increase in the number and clearness of the lines, upon which the shape of the hole was also found to have an effect."

A STANDARD METHOD OF STEAM-BOILER TRIALS.

Report of Committee to the American Society of Mechanical Engineers.

From Advanced Copy of Transactions of the American Society of Mechanical Engineers.

I.

1. The importance of establishing a method of trial of steam-boilers that should determine their steaming capacity under any given set of conditions, and their economy in the use of fuel is so thoroughly understood and so definitely recognized by engineers engaged in the design and construction, or management and use, of them, that it has been thought, by all, that some system of testing should be settled upon, for general use, which may be relied upon to give all the facts needed in relation to the performance of boilers, with substantial accuracy, and yet with least possible expenditure of time and money, and a method which may be adopted by any fairly skillful engineer, without the use, so far as it can be avoided, of unusual forms of apparatus.

It has been the duty of your committee to examine carefully the methods of testing boilers now practiced, to consider to what extent they present advantages or disadvantages, and finally to frame a Code of Instructions, embodying what they consider to be the best methods of experiment and the most satisfactory plan of working up and stating results. In this labor they have met with all the difficulties which usually attend an attempt to reconcile the opposing views of those who are acknowledged to be authorities on the subject, and to combine the various advantages possessed by systems in use among such members of the profession. Their object has been, not to prescribe a regulation method of test that shall be considered as representing the most complete possible system, and as giving results exact to the degree that would be satisfactory in purely scientific work, but to propose a code for daily use by the practising engineer which may be relied upon for substantial accuracy, to limits of error within the range of commercial requirements, one that may be adopted by any engineer deserving of a place within the ranks of the profession, and one that may be followed closely under ordinary circumstances of everyday experience.

It has, however, also been attempted to present, independently, a view of the refinements of recent practice in this matter which may be of service to the engineer who finds it desirable and possible to attempt work of scientific exactness, and of the utmost possible completeness.

2. The object of a trial of a steam-boiler, as your committee understands it, is to determine with great precision what is the quantity of steam that a boiler can supply continuously and regularly under definitely prescribed conditions; what is the condition, and therefore the commercial value, of that steam; what is the amount of fuel demanded to produce that steam supply; what is the character of the combustion, and what are the actual conditions of operation of the boiler when at work all of which should be presented in a report stating the results thus determined. The conditions prescribed for one trial may differ greatly from those demanded for another trial of the same, or of another boiler, and those differences of circumstances are often the essential matters to be studied, and their effect noted upon the performance of the boiler which is the subject of the report. In any case, however, it is assumed that the conditions under which the boiler is to be worked are to be definitely stated, and the engineer conducting the experiments is expected to ascertain as exactly as possible the facts which go to determine the performance of the boiler, and to state them with accuracy, conciseness and thoroughness.

In the attempt to ascertain those facts by observation of the actual performances of the boiler, the engineer meets with some serious difficulties, and finds it necessary to use the most perfect apparatus, and to exercise the utmost care and skill. In even so simple a matter as the weighing of coal and the measure-

ment of water, errors are often found where least expected, and they may make their appearance even in the work of painstaking and experienced practitioners. In conducting a steam-boiler trial, the weight of the water supplied to the boiler must be exactly determined; the weight of the fuel consumed must be similarly obtained; the state of the steam made must be determined, and those quantities must be noted at such frequent intervals, during the test, that the log will exhibit every irregularity of operation, and its effect upon the performance of the apparatus. To secure thoroughly satisfactory results, it is also necessary to know whether the combustion is perfect or imperfect, and to what extent the character of the combustion, as well as the other conditions and facts noted, are due to the excellences or the defects of the boiler, and what to external conditions.

3. In the tests of boilers made in earlier times, these determinations were made with comparative crudeness of method, and the results of such methods were such as would be considered to-day grossly inaccurate. The coal consumed was in large part estimated, and no pains were taken to ascertain the amount of unevaporated water carried over with the steam. It thus often happened that results were reported that were far beyond the utmost possible efficiency; the evaporation of water was sometimes reported at a higher figure than theoretical perfection would yield; and it has only been within a very recent period that it has been possible to judge what is the real performance of the standard types of steam-boiler, under ordinary circumstances, from the reports published, in many cases, as the work of engineers of reputation.

A great change has been gradually taking place both in the sentiments, and in the practice, of engineers engaged in this department of professional work, and it has come to be considered that the exact determination of power and economy of a steam-boiler demands the exercise of all the care, skill and perfection of method, and of apparatus required in the prosecution of any purely scientific investigation. It is now demanded that the weights of fuel and of water, the perfection of the combustion, the quality of the steam, and the temperatures of feed water and of furnace flue, shall be determined with an accuracy that shall be within the limits of error of good instruments; that, wherever possible, a system shall be adopted which shall permit of checking and verification of the reported results, and which shall make it as nearly as possible certain that no error can enter the work without prompt detection and correction. It is further demanded that all important work of this kind shall be done in substantially the same way, in order that comparisons may be easily made without the necessity of going through long and troublesome calculations in the effort to reduce the reports to be compared to a common basis.

4. This sentiment, and these demands, can evidently be complied with only by the establishment of some standard unit of measure of the power of the boiler, and of evaporative efficiency, and some definite and standard method of conducting the test. This standard unit of measure must be simple, easily defined, and convenient in application; the standard method of trial of boilers must be prescribed by a code of rules so concise, and yet so definite, that every member of the profession may be able to adopt them. The scheme must also be so complete that, if carefully and exactly followed, the precise value of the boiler may be ascertained with certainty. The method of record of facts determined must be such as will exhibit all the essential quantities in tabular form, and unobscured by the introduction of unessential figures.

5. Such a code of rules has been proposed by a joint committee of the Union of German Engineers and of the Central Union of Associations for the care of Steam-Boilers, and this set of regulations may be considered as the embodiment of the best ideas of our Continental colleagues on this subject. Your committee have examined this document with care, and find themselves in full accord with its proposers in the main, while obliged to offer some modifications of the scheme which are thought to make it more effective and more acceptable to American engineers. The Code of Rules for Use in Trials of Steam-Boilers which your committee proposes is herewith submitted

and will be found appended to this report.

6. The first provision of the code is that the object of the test to be made shall be precisely stated, and carefully kept in view during the whole trial, and during the preparation of the report. This object may be the determination of the steaming capacity, of the maximum efficiency, or of the quality of steam supplied by the boiler under specified conditions; or it may be the comparison of the qualities of various fuels. These objects cannot all be attained at one time, and maximum steaming capacity and maximum economy of fuel are, almost invariably, if not always, the result of incompatible conditions. The method of handling the steam generator will therefore differ as one or the other of these objects is to be sought.

It is next provided that the boiler to be tested shall be exactly measured, in order that data may be obtained for subsequent calculations. These measurements should be taken before the trial, not only because that is usually the most convenient time, but also because this preliminary measurement may sometimes lead to the discovery of defects of construction, as well as of proportions, that may suggest modifications of the plan of test previously laid down. The boiler is then to be put in the best possible order, in every respect, so that its observed merits or defects may not be obscured by accidental conditions having no relation to such merits or defects.

7. It is provided that an understanding shall be reached, before the trial, in regard to the kind of fuel to be used. Neglect of this precaution sometimes leads to needless misunderstandings, and avoidable criticism of the results reported. It is proposed that, where no reason of controlling importance exists to the contrary, the best obtainable coal shall be selected, for the reason that it is thought that a boiler can be better judged, and the results of its trial may be more satisfactorily compared with similar trials of other boilers when the very best work of which it is capable is done by it. The differences between separate lots of the best coals are less than the differences between separate lots of inferior fuels, and the comparison is thus less difficult where the former are used. To secure still more exact knowledge of the influence of the quality of the fuel upon the performance of the boiler, it is considered advisable to have an analysis made of the coal used, in all cases in which it can be done.

8. The establishment of the correctness of all the apparatus to be employed in the test is the first of the preliminaries to their use. The standardization of the instruments is a matter of supreme importance, since upon their accuracy the whole work of the engineer is dependent. It is also a work demanding, in most cases, unusual skill and care, and, to be satisfactory, must generally be performed either at the manufacturer's or at the office of the engineer conducting the trial. The scales can usually be standardized by the official sealer of weights and measures, and sealed by him; the water meters, if used, can be readily tested by the use of the scales so sealed; the thermometers are, as a rule, best tested by their makers, and should be sent to the maker for test immediately before, and directly after, the test, The engineer often has a carefully preserved standard with which they may be compared in his own office. The same remarks apply to the examination of the gauges used, which should be standardized both before and after their use. The apparatus used in connection with the calorimeter, in the determination of the quality of the steam made, demand exceptional care in this process; they are rarely of sufficient delicacy and accuracy to give perfectly satisfactory single determinations, even at the best, and the use of ordinary commercial instruments, carelessly standardized, or not at all, cannot be too strongly deprecated. Where it is unavoidable, the use of coarsely graduated thermometers and roughly constructed scales may be permitted, but only then when a very large number of observations are taken, and an average thus obtained which may be fairly expected to fall within reasonable limits of error—say within one per cent.

9. The precautions to be taken before beginning a trial are prescribed in some detail, since your committee consider them of great importance, and have known of serious embarrassment arising by their neglect.

The method of starting and of stop-

ping the trial is prescribed in a form which seems to your committee best as a whole. This is a very important matter, and yet is one upon which engineers of experience and acknowledged authority are not in complete accord. Your committee, for this, and also for the other reason, that the plan here proposed may not be always practicable, prescribe a second or alternative method, which may be adopted for such cases, or, where the engineer conducting the test is confident of being able to do better work than by the first of the two methods. The principles to be adhered to in this matter, as in every other detail of the operation of testing a boiler, are easily specified, but they are not always as easy of practice. All conditions should be as exactly the same at the beginning and at the end of the test as they can possibly be made. The period of the trial, and the times of stopping and of starting, should be capable of being exactly fixed, and the method of test should be such as should permit of the commencement and the end occurring at these exactly defined times, or, as an alternative, they should be such that the work done by the boiler during the less precisely determinable time of beginning and ending of the trial should be as nearly as possible *nil*, so that a slight error as to time may not appreciably affect the results. The "Standard Method" proposed by your committee is considered to meet these requirements as fully as any method in use. The alternative method is regarded as the next best.

10. During the trial, the essential provision should be the preservation of the utmost possible uniformity of working conditions throughout the whole period of the trial. Every irregularity gives rise to more or less loss of efficiency and to uncertainty in regard to the correctness of the reported figures. The nearer the working of the boiler is kept to the final average for the trial, the better.

11. Your committee consider the method of keeping the record of the test as no less important than the method of test itself. Perfect uniformity of operation within the boiler-room, and maximum efficiency of boiler, are best attainable where a system of record is adopted which allows of that regularity being shown at all times; and records in proper form are the best possible security against errors of observation. The committee are unanimous in recommending that graphical methods be adopted wherever it is found practicable to employ them. Such methods of record also exhibit most satisfactorily the accordance with, or the deviation from, the uniformity of operation considered so desirable on the score of efficiency and accuracy. Your committee present a form of record-blank which they consider as concise as is ever desirable in any important trial; and would prefer, in special cases, a more, rather than a less, complete record.

12. It is proposed by your committee as desirable that, when practicable, analyses of the escaping gases should be made. This is an operation of great simplicity, and can easily be made familiar to any engineer who chooses to take the trouble of learning it. If, for any reason, it is not found convenient to make the analysis in the office of the engineer, he can readily have the work done, at little expense, by intrusting his samples to a chemist of known skill and reliability. This provision is made as a part of the code, on the ground that it is only by a knowledge of the proportion of the constituents of the flue-gases that it can be determined whether the combustion is complete, whether the products of combustion are diluted with excess of air, and whether the fuel used has been so burned as to give its best effect. Such analyses also enable the engineer to ascertain the best method of burning the fuel. The code prescribes the precautions to be taken when this detail is carried into effect.

13. The establishment of the value of the "Unit of Evaporation," and that of the "Commercial Horse-Power" of the boiler, are matters which have been considered by your committee to be of essential importance to the settlement of a thoroughly complete standard method of trial, and of a perfectly satisfactory system of reporting results.

It has been evident to every observer that the sentiment above alluded to, as having arisen among engineers during the present generation, in favor of reducing the whole matter of testing boilers to an acknowledged standard system, has led to the endeavor, on the part of

the most able among practitioners, to determine standards with which to compare results obtained in such trials. The two most essential standards are those just referred to. The trials of boilers are made under a wide range of actual conditions, the steam pressure, the temperature of feed-water, the rate of combustion and of evaporation, and, in fact, every other variable condition, differing in any two trials to such an extent that direct comparison of the totals obtained, as a matter of information relating to the relative value of the boilers, or of the fuel used, becomes out of the question. It has thus gradually come to be the custom to reduce all results to the common standard of weight of water evaporated by the unit weight of fuel, the evaporation being considered to have taken place at mean atmospheric pressure, and at the temperature due that pressure, the feed-water being also assumed to have been supplied at that temperature. This is, in technical language, said to be the "equivalent evaporation from and at the boiling point" (212° Fahr.). This standard has now become so generally and so indisputably incorporated into the science and the practice of steam engineering that your committee, even were they acquainted with any other equally satisfactory unit, would hesitate to recommend anything else. They would simply express their approval of the adoption, and recommend the permanent retention, of this, which, as has been previously proposed, they would denominate the "*Unit of Evaporation*," *i. e.*, one pound of water at 212° F. evaporated into steam of the same temperature. This is equivalent to the utilization of 965.7 British thermal units per pound of water so evaporated. The relative economy of the boiler would then, as is customary, be expressed by the number of units of evaporation obtained per pound of combustible.

14. The character and magnitude of the unit to be chosen to express the "power" of the steam-boiler is not as well settled; and your committee find themselves compelled to take up, in this matter, a subject which has attracted much attention among engineers, and which remains nevertheless, unsettled. It is evident that, since the boiler is simply an apparatus for the generation of steam, and since the province of the steam-engine is to develop power from that steam, by the conversion of heat into mechanical energy; and since, furthermore, the engine develops power with a degree of efficiency which may vary enormously with differences in construction and operation of that machine, it cannot be properly said that we have any natural unit of power for rating steam-boilers. The most nearly scientific system of power rating yet proposed is perhaps that which considers the power of a boiler to be that expended by it in driving all the steam which it makes out against the pressure of the atmosphere, a system which does not, however, meet the wants of engineers. What is needed is a standard unit of boiler-power which may be used commercially in rating boilers, and in specifications prescribing the power to be demanded by the purchaser and guaranteed by the vender. It is evident that such a unit would not, if established, serve as a gauge of the power to be actually obtained from any given combination of engine and boiler, since the power so obtained must be measured by the indicator at the engine, and not at the boiler, and since in so measuring power, the economy and efficiency of the boiler would be elements left entirely out of the account. The best that can be done is obviously to assume a set of practically attainable conditions under which it would be fair to assume that the boiler may be properly expected to be operated in average good practice, and to take the power so obtainable as the measure of its power to be used in commercial and engineering transactions. The unit which has been most generally assumed, up to the present time, is the weight of steam demanded per horse-power per hour by a fairly good steam-engine. The magnitude of this quantity has been gradually and constantly decreasing from the earliest period of the history of the steam-engine. In the time of Watt, one cubic foot of water per hour per horse-power was thought a fair allowance; at the middle of the present century, ten pounds of coal was still not an unusual figure for the consumption per hour per horse-power, and five pounds, equivalent to about forty pounds of feed-water, was a good allowance for the best engines.

After the introduction of the modern forms of expansively working engines, this last figure was reduced twenty-five per cent., and the most recent improvements have still further lessened the consumption of fuel and of steam. By general consent, it seems likely that the unit which will meet with final acceptance for general purposes, in the estimation of boiler-power, is not far from thirty pounds of dry steam per horse-power per hour. This represents the performance of good mill engines of the non-condensing type. Large engines, with condensers, or compounded cylinders, will do better by from twenty to thirty per cent. Your committee have concluded to recommend thirty pounds as the unit of boiler-power.

15. But it remains to be determined under what circumstances this figure shall be taken as standard. It is on this subject that practitioners, and the members of your committee, as well, are not fully agreed. Nevertheless, it is, in their opinion, advisable that some definite set of conditions be prescribed to be taken as standard without waiting for complete accordance of opinion throughout the profession.

The Committee of Judges of the Centennial Exhibition, to whom the trials of competing boilers at that exhibition were intrusted, met with this same problem, and finally agreed to solve it, at least so far as the work of that committee was concerned, by the adoption of the unit, 30 *pounds of water evaporated into dry steam per hour from feed water at* 100° *Fahrenheit, and under a pressure of seventy pounds per square inch above the atmosphere*, these conditions being considered by them to represent fairly average practice. The quantity of heat demanded to evaporate a pound of water under these conditions is 1110.2 British thermal units, or 1.1496 units of evaporation (such as are here adopted and proposed for general use). The unit of power proposed is thus equivalent to the development of 33,305 heat units per hour, or 34,488 units of evaporation. The arguments in favor of the retention of this unit of power without modification are: (1) It is, to a certain extent, established, being the only unit proposed by authority, up to the present time, which has been accepted to any important extent by practitioners. (2) It is considered by its proposers, and probably by engineers generally, to fairly represent good average practice in the application of steam-power, as exhibited in the operation of engines and boilers under ordinary actual working conditions. Both of these arguments are deemed by your committee to be valid and deserving of careful consideration. The abandonment of an already established standard is always confusing, and should not be permitted without the most cogent of reasons.

Another standard unit, which has been proposed to your committee, and strongly urged as preferable to the above, is that represented by the evaporation of thirty pounds of feed-water into dry steam "*from and at the boiling point*," at mean atmospheric pressure (212° F.). The arguments in favor of this unit are the following: (1) In the determination of the unit of evaporation to be used in steam-boiler practice, it has been generally, and probably unanimously, decided by engineers that the evaporation shall be reckoned as having been effected at the boiling point from water assumed also to be supplied at that temperature, and that one pound thus evaporated shall be the unit. This being the established unit of evaporation, consistency and convenience both dictate that the power of the boiler should be expressed in the same unit, or some handy multiple thereof; (2) It is submitted that the reduction of this unit to an exact multiple of the unit of evaporation will greatly facilitate calculations, inasmuch as the work done by the boiler is to be reduced to the same standard of feed-temperature and temperature of evaporation; (3) By the adoption of this unit, the trouble and risk of error coming from the attempt to use a factor as proposed above, differing from the multiple of the already accepted factor by 14.96 per cent., may be entirely avoided; (4) The unit last proposed is equivalent to 26.09 pounds of water evaporated from 100° Fahr. into steam at 70 pounds pressure, and is claimed to be itself more nearly representative of good average practice than the centennial unit.

Your committee has carefully weighed the arguments relating to these standards, as they were presented in writing

by their respective advocates, and, after due consideration, has determined to accept the Centennial Standard, the first above mentioned,, and to recommend that in all standard trials the commercial horse-power be taken as *an evaporation of* 30 *pounds of water per hour from a feed-water temperature of* 100° *Fahr. into steam at* 70 *pounds gauge pressure*, which shall be considered to be equal to 34½ units of evaporation, that is, to 34½ pounds of water evaporated from a feed-water temperature of 212° Fahr. into steam at the same temperature. This standard is equal to 33,305 thermal units per hour.*

It is the opinion of this committee that a boiler rated at any stated number of horse-powers should be capable of developing that power with easy firing, moderate draught and ordinary fuel, while exhibiting good economy; and further, that the boiler should be capable of developing at least one-third more than its rated power to meet emergencies at times when maximum economy is not the most important object to be attained.

Any increase of temperature derived from a feed-water heater acted upon by the products of combustion escaping from a boiler should not be credited to the evaporative efficiency of the boiler, except by agreement; and in the latter case accurate tests can be made only with feed-water of the average temperature used during the regular operation of the boiler.

The code presented by your committee is necessarily, as has been already indicated, condensed to the utmost possible extent consistent with exactness, and essential completeness. In matters of detail, it must be left to the engineer to carry out the evident spirit and intent of the code by devising his own methods; and it may be expected that every engineer will be competent to supplement the directions here given, as far as is necessary.

* According to the tables in Porter's "Treatise on the Richards Steam-Engine Indicator," which tables the committee would recommend for general acceptance by engineers, an evaporation of 30 pounds of water from 100° F., into steam at 70 pounds pressure is equal to an evaporation of 34.488 pounds from and at 212°; and an evaporation of 34½ pounds from and at 212° F., is equal to 30.010 pounds from 100° F., into steam at 70 pounds pressure.

The "unit of evaporation" being equal to 965.7 thermal units, the commercial horse-power=34,488×965.7 =33,305 thermal units.

In order, however, to exhibit the extent to which he may work up such details, and to present the views of the members of the committee more fully, both in matters in which they agree and in those in which differences of views exist, an appendix is added to the report, in which memoranda written out by them are given describing details of work more fully than they are given in the code, and expressing individual opinions in regard to such matters as have seemed to each of such importance as to demand special notice. Each of these notes is signed with the initials of the writer.

Respectfully submitted:

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J. C. Hoadley,
R. H. Thurston,
Chas. E. Emery,
Chas. T. Porter,
} *Committee.*

Code of Rules for Boiler Tests.

PRELIMINARIES TO A TEST.

I. *In preparing for* and conducting trials of steam-boilers, the specific object of the proposed trial should be clearly defined and steadily kept in view. (Appendix 1.)

II. *Measure and record the dimensions*, position, etc., of grate and heating surfaces, flues and chimneys, proportion of air space in the grate surface, kind of draught, natural or forced.

III. *Put the boiler in good condition.* Have heating surface clean inside and out, grate bars and sides of furnace free from clinkers, dust and ashes removed from back connections, leaks in masonry stopped, and all obstructions to draught removed. See that the damper will open to full extent, and that it may be closed when desired. Test for leaks in masonry by firing a little smoky fuel and immediately closing damper. The smoke will then escape through the leaks.

IV. *Having an understanding with the parties* in whose interest the test is to be made as to the character of the coal to be used. The coal must be dry, or, if wet, a sample must be dried carefully and a determination of the amount of moisture in the coal made, and the calculation of the results of the test corrected accordingly.

Wherever possible, the test should be made with standard coal of a known quality. For that portion of the country east of the Allegheny Mountains good anthracite egg coal or Cumberland semi-bituminous coal may be taken as the standard for making tests. West of the Allegheny Mountains and east of the Missouri River, Pittsburgh lump coal may be used*

V. *In all important tests* a sample of coal should be selected for chemical analysis.

VI. *Establish the correctness of all apparatus* used in the test for weighing and measuring. These are:

1. Scales for weighing coal, ashes and water.
2. Tanks, or water meters for measuring water. Water meters as a rule should only be used as a check on other measurements. For accurate work, the water should be weighed or measured in a tank. (Appendix VI. and VII.)
3. Thermometers and pyrometers for taking temperatures of air, steam, feed-water, waste gases, etc. (Appendix X. to XIII.)
4. Pressure gauges, draft gauges, etc. (Appendix IX, XIV, and XV.)

VII. *Before beginning a test*, the boiler and chimney should be thoroughly heated to their usual working temperature. If the boiler is new, it should be in continuous use at least a week before testing, so as to dry the mortar thoroughly and heat the walls.

VIII. Before beginning a test, the boiler and connections should be free from leaks, and all water connections, including blow and extra-feed pipes, should be disconnected or stopped with blank flanges, except the particular pipe through which water is to be fed to the boiler during the trial. In locations where the reliability of the power is so important that an extra feed pipe must be kept in position, and in general when for any other reason water pipes other than the feed pipes cannot be disconnected, such pipes may be drilled so as to leave openings in their lower sides, which should be kept open throughout the test as a means of detecting leaks, or accidental or unauthorized opening of valves. During the test the blow-off pipe should remain exposed.

If an injector is used it must receive steam directly from the boiler being tested, and not from a steam pipe, or from any other boiler.

See that the steam pipe is so arranged that water of condensation cannot run back into the boiler. If the steam pipe has such an inclination that the water of condensation from any portion of the steam-pipe system may run back into the boiler, it must be trapped so as to prevent this water getting into the boiler without being measured.

STARTING AND STOPPING A TEST.

IX. A test should last at least ten hours of continuous running, and twenty four hours whenever practicable. The conditions of the boiler and furnace in all respects should be, as nearly as possible, the same at the end as at the beginning of the test. The steam pressure should be the same, the water level the same, the fire upon the grates should be the same in quantity and condition, and the walls, flues, etc., should be of the same temperature. To secure as near an approximation to exact uniformity as possible in conditions of the fire and in temperatures of the walls and flues, the following method of starting and stopping a test should be adopted:

X. *Standard Method.*—Steam being raised to the working pressure, remove rapidly all the fire from the grate, close the damper, clean the ash pit, and as quickly as possible start a new fire with weighed wood and coal, noting the time of starting the test and the height of the water level while the water is in a quiescent state, just before lighting the fire.

At the end of the test, remove the whole fire, clean the grates and ash pit, and note the water level when the water is in a quiescent state; record the time of hauling the fire as the end of the test. The water level should be as nearly as possible the same as at the beginning of the test. If it is not the same, a correction should be made by computation, and not by operating pump after test is completed. It will generally be necessary to

* These coals are selected because they are about the only coals which contain the essentials of excellence of quality, adaptability to various kinds of furnaces, grates, boilers, and methods of firing, and wide distribution and general accessibility in the markets.

regulate the discharge of steam from the boiler tested by means of the stop valve for a time while fires are being hauled at the beginning and at the end of the test, in order to keep the steam pressure in the boiler at those times up to the average during the test.

XI. *Alternate Method.*—Instead of the Standard Method above described, the following may be employed where local conditions render it necessary.

At the regular time for slicing and cleaning fires have them burned rather low, as is usual before cleaning, and then thoroughly cleaned; note the amount of coal left on the grate as nearly as it can be estimated; note the pressure of steam and the height of the water level—which should be at the medium height to be carried throughout the test—at the same time; and note this time as the time of starting the test. Fresh coal, which has been weighed, should now be fired. The ash pits should be thoroughly cleaned at once after starting. Before the end of the test the fires should be burned low, just as before the start, and the fires cleaned in such a manner as to leave the same amount of fire, and in the same condition, on the grates as at the start. The water level and steam pressure should be brought to the same point as at the start. The water level and steam pressure should be brougnt to the same point as at the start, and the time of the ending of the test should be noted just before fresh coal is fired.

DURING THE TEST.

XII. *Keep the Conditions Uniform.*—The boiler should be run continuously, without stopping for meal times or for rise or fall of pressure of steam due to change of demand for steam. The draught being adjusted to the rate of evaporation or combustion desired before the test is begun, it should be retained constant during the test by means of the damper.

If the boiler is not connected to the same steam-pipe with other boilers, an extra outlet for steam with valve in same should be provided, so that in case the pressure should rise to that at which the safety valve is set, it may be reduced to the desired point by opening the extra outlet, without checking the fires.

If the boiler is connected to a main steam-pipe with other boilers, the safety valve on the boiler being tested should be set a few pounds higher than those of the other boilers, so that in case of a rise in pressure the other boilers may blow off, and the pressure be reduced by closing their dampers, allowing the damper of the boiler being tested to remain open, and firing as usual.

All the conditions should be kept as nearly uniform as possible, such as force of draught, pressure of steam, and height of water. The time of cleaning the fires will depend upon the character of the fuel, the rapidity of combustion, and the kind of grates. When very good coal is used, and the combustion not too rapid, a ten-hour test may be run without any cleaning of the grates, other than just before the beginning and just before the end of the test. But in case the grates have to be cleaned during the test, the intervals between one cleaning and another should be uniform.

XIII. *Keeping the Records.*—The coal should be weighed and delivered to the firemen in equal portions, each sufficient for about one hour's run, and a fresh portion should not be delivered until the previous one has all been fired. The time required to consume each portion should be noted, the time being recorded at the instant of firing the first of each new portion. It is desirable that at the same time the amount of water fed into the boiler should be accurately noted and recorded, including the height of the water in the boiler, and the average pressure of steam and temperature of feed during the time. By thus recording the amount of water evaporated by successive portions of coal, the record of the test may be divided into several divisions, if desired, at the end of the test, to discover the degree of uniformity of combustion, evaporation and economy at different stages of the test. (Appendix II. and III.)

XIV. *Priming Tests.*—In all tests in which accuracy of results is important, calorimeter tests should be made of the percentage of moisture in the steam, or of the degree of superheating. At least ten such tests should be made during the trial of the boiler, or as many as to reduce the probable average error to less than one per cent., and the final records of the boiler test corrected according to

the average results of the calorimeter tests.

On account of the difficulty of securing accuracy in these tests, the greatest care should be taken in the measurements of weights and temperatures. The thermometers should be accurate to within a tenth of a degree, and the scales on which the water is weighed to within one hundredth of a pound. (Appendix XVII. to XXI.

XV. ANALYSES OF GASES.—MEASUREMENT OF AIR SUPPLY, ETC.

In tests for purposes of scientific research, in which the determination of all the variables entering into the test is desired, certain observations should be made which are in general not necessary in tests for commercial purposes. These are the measurement of the air supply, the determination of its contained moisture, the measurement and analysis of the flue gases, the determination of the amount of heat lost by radiation, of the amount of infiltration of air through the setting, the direct determination by calorimeter experiments of the absolute heating value of the fuel, and (by condensation of all the steam made by the boiler) of the total heat imparted to the water.

The analysis of the flue gases is an especially valuable method of determining the relative value of different methods of firing, or of different kinds of furnaces. In making these analyses great care should be taken to procure average samples—since the composition is apt to vary at different points of the flue—(Appendix XVI.)—and the analyses should be intrusted only to a thoroughly competent chemist, who is provided with complete and accurate apparatus.

As the determinations of the other variables mentioned above are not likely to be undertaken except by engineers of high scientific attainments, and as apparatus for making them is likely to be improved in the course of scientific research, it is not deemed advisable to include in this code any specific directions for making them.

XVI. RECORD OF THE TEST.

A "log" of the test should be kept on properly prepared blanks, containing headings as follows:

Time.	Pressures.			Temperatures.					Fuel.		Feed Water.	
	Barometer.	Steam Gauge.	Draught Gauge.	External Air.	Boiler Room.	Flue.	Feed Water.	Steam.	Time.	lbs.	Time.	lbs. or cubic ft.

XVII. REPORTING THE TRIAL.

The final results should be recorded upon a properly prepared blank, and include as many of the following items as are adapted for the specific object for which the trial is made. The items marked with a * may be omitted for ordinary trials, but are desirable for comparison with similar data from other sources.

Results of the trials of a.....
boiler at.....
To determine.....

1. Date of trial			
2. Duration of trial	hours.		
DIMENSIONS AND PROPORTIONS.			
Leave space for complete description. (See Appendix XXIII.)			
3. Grate surface .....wide.....long.....area	sq. ft.		
4. Water heating surface	sq. ft.		
5. Superheating surface	sq. ft.		
6. Ratio of water heating surface to grate surface			
AVERAGE PRESSURES.			
7. Steam pressure in boiler, by gauge	lbs		
*8. Absolute steam pressure	lbs.		
*9. Atmospheric pressure per barometer	in.		
10. Force of draught in inches of water	in.		
AVERAGE TEMPERATURES.			
*11. Of external air	deg.		
*12. Of fire-room	deg.		
*13. Of steam	deg.		
14. Of escaping gases	deg.		
15. Of feed-water	deg.		
FUEL.			
16. Total amount of coal consumed†	lbs.		
17. Moisture in coal	per cent.		
18. Dry coal consumed	lbs.		
19. Total refuse, dry.....pounds =	per cent.		
20. Total combustible (dry weight of coal, Item 18, less refuse, Item 19)	lbs.		
*21. Dry coal consumed per hour	lbs.		
*22. Combustible consumed per hour	lbs.		
RESULTS OF CALORIMETRIC TESTS.			
23. Quality of steam, dry steam being taken as unity			
24. Percentage of moisture in steam	per cent.		
25. No. of degrees superheated	deg.		
WATER.			
26. Total weight of water pumped into boiler and apparently evaporated‡	lbs.		
27. Water actually evaporated, corrected for quality of steam§	lbs.		
28. Equivalent water evaporated into dry steam from and at 212° F.	lbs.		
*29. Equivalent total heat derived from fuel in British thermal units§§	B. T. U.		
30. Equivalent water evaporated into dry steam from and at 212° F. per hour	lbs.		
ECONOMIC EVAPORATION.			
31. Water actually evaporated per pound of dry coal, from actual pressure and temperature§	lbs.		
32. Equivalent water evaporated per pound of dry coal from and at 212° F.	lbs.		
33. Equivalent water evaporated per pound of combustible from and at 212° F.	lbs.		
COMMERCIAL EVAPORATION.			
34. Equivalent water evaporated per pound of dry coal, with one-sixth refuse, at 70 pounds gauge pressure, from temperature of 100° F.=Item 33 multiplied by 0.7249	lbs.		
RATE OF COMBUSTION.			
35. Dry coal actually burned per sq. foot of grate surface per hour	lbs.		
*36. (Consumption of dry coal) Per sq. ft. of grate surface	lbs.		
*37. (per hour. Coal assumed) Per sq. ft. of water heating surface	lbs.		
*38. (with one-sixth refuse.§) Per sq. ft. of least area for draught.	lbs.		

	RATE OF EVAPORATION.			
39.	Water evaporated from and at 212° F. per sq. ft. of heating surface per hour		lbs.	
*40.	Water evaporated per hour from temperature of 100° F. into steam of 70 pounds gauge pressure.§	Per sq. ft. of grate surface	lbs.	
*41.		Per sq. ft. of water heating surface	lbs.	
*42.		Per sq. ft. of least area for draught	lbs.	
	COMMERCIAL HORSE-POWER.			
43.	On basis of thirty pounds of water per hour evaporated from temperature of 100° F. into steam of 70 pounds gauge pressure, (=34½ lbs. from and at 212°)§		H. P.	
44.	Horse-power, builders' rating at . . . square feet per horse-power		H. P.	
45.	Per cent. developed above, or below, rating§		per cent.	

* See reference in paragraph preceding table.

† Including equivalent of wood used in lighting fire. 1 pound of wood equals 0.4 pound coal. Not including unburnt coal withdrawn from fire at end of test.

‡ Corrected for inequality of water level and of steam pressure at beginning and end of test.

§ The following shows how some of the items in the above table are derived from others:

Item 27 = Item 26 × Item 23.

Item 28 = Item 27 × Factor of evaporation.

Factor of evaporation $= \frac{H-h}{965.7}$, H and h being respectively the total heat units in steam of the average observed pressure and in water of the average observed temperature of feed, as obtained from tables of the properties of steam and water.

Item 29 = Item 27 × (H−h).

Item 31 = Item 27 ÷ Item 18.

Item 32 = Item 28 ÷ Item 18 or = Item 31 × Factor of evaporation.

Item 33 = Item 28 ÷ Item 20 or = Item 32 ÷ (per cent. 100−Item 19).

Items 36 to 38. First term = Item 20 × $\frac{8}{9}$.

Items 40 to 42. First term = Item 39 × 0.8698.

Item 43 = Item 29 × 0.00003 or $= \frac{\text{Item 30}}{34\frac{1}{2}}$.

Item 45 $= \frac{\text{Difference of Items 43 and 44}}{\text{Item 44.}}$

ON THE RESISTANCE OF LOCOMOTIVES AND TRAINS—THE EFFICIENCY OF LOCOMOTIVES AND THEIR CONSUMPTION OF WATER AND FUEL.

By Professor A. FRANK.

Translated from "Organ für die Fortschritte des Eisenbahnwesens," for Abst. of the Inst. of Civil Engineers.

The first is a theoretical paper, but entirely founded on practical experiments. The author refers first to the formulas given by Messrs. Guillemin, Guebhard and Dieudonné, in their work on the resistance of trains (Paris, 1868,) and by Von Röckl, of the Bavarian State Railways, in the "Organ für die Fortschritte des Eisenbahnwesens" for 1880. The formulas of the former authors are as follows:—

1*a*. For goods trains with speeds of 12 to 32 kilos per hour, and oil lubrication.

$$r = 1.65 + 0.05\ V.$$

2*a*. Passenger trains with speeds of 32 to 50 kilometers per hour.

$$r = 1.8 + 0.08\ V + \frac{0.009\ F\ V^2}{Q}.$$

3*a*. Passenger trains with speeds of 50 to 65 kilometers per hour.

$$r = 1.8 + 0.08\,V + \frac{0.006\,F\,V^2}{Q}$$

4*a* Express trains with speeds of 70 kilometers per hour and upwards.

$$r = 1.8 + 0.14\,V + \frac{0.004\,F\,V^2}{Q}.$$

Here V is the speed in kilometers per hour, F is the end section of the carriages (taken at 5 square meters), Q is the total weight of the train, excluding engine and tenders in tonnes, and r is the resistance in kilograms per tonne. For locomotives the results of experiments were given, but no formula was developed.

Von Röckl's formulas are as follows:—

1*b*. For Locomotives,

$$k_1 = 0.005 + 0.00000021\,V^3.$$

2*b*. For Carriages,

$$k_2 = 0.0025 + 0.00000021\,V^3.$$

Here V is the speed in kilometers per hour, and k_1 k_2 are the resistances in kilograms per kilogram of gross weight.

These formulas all refer to a straight horizontal track; on a curve the coefficients in 1*b* and 2*b* are to be increased by k_3, where

$$k_3 = \frac{0.6504}{R - 55}.$$

Here R is the radius of the curve in meters.

The writer first considers the case of a locomotive and tender, without a train, running on a straight horizontal road, not under steam and so slowly that the resistance of the air may be neglected. The sources of resistance are then,

1. The sliding friction of all the moving parts;
2. The rolling friction between the wheels and the rails.

The amount of the former depends chiefly on the load, on the distance moved by the sliding parts as compared with the engine as a whole, and on the coefficient of friction for the sliding pressures. It is greatly influenced by the nature of the lubricants and the care in lubrication. It is less as the wheel-diameter is larger, and is generally less, therefore, in passenger than goods engines. On the whole it may be expressed by m Q, where Q is the weight, and m a coefficient determined by experiment.

If the locomotive be now supposed to be under steam, there will be a further resistance, due to the increased friction of the slides, etc. This will be independent of the speed, and may be denoted by S. If the speed is increased, the resistance of the air must be taken into account; this will be represented by the following equation:

$$p = l\,F\,V^2,$$

where F is the surface meeting the air, and l a coefficient determined by experiment. Next must be considered the resistance arising from unevenness in the road; this will produce alternate risings and fallings of the wheels, bendings of the springs, and variations in the friction of the axles, &c. Assuming that the vertical motion varies as the square of the speed, the resistances thus produced will vary in the same proportion. The same will be true of the resistances occasioned by horizontal oscillations. Finally there may be shocks due to flat places in the tires, or in elastic portions of the rails. The resistances thus occasioned may also be taken to vary as the square of the speed. The same will apply exactly to the case of a carriage or wagon, except as relates to the resistance occasioned by the steam. Finally the increase in this latter resistance, due to the engine having a train attached, may be considered proportional to the total work to be done. If, then, this increase be denoted by the letter i, the final equation for the resistance W of a complete train will be of the form

$$W = (1 + i)\,(m\,Q + S + B\,V^2)$$

This equation does not agree with any of those given at the beginning. Those of Guillemin contain a term involving V, which is the reason why they are obliged to give different formulas for different speeds; but if V be put equal to nothing, values will be obtained for the resistance (neglecting that of the air) which are much too small, and which moreover are less for goods than for passenger trains. On the other hand Von Rockl's experiments contain no term varying as V, but have a term varying as V^3. This cannot be correct, as neither the resistance of the air nor that due to shocks varies in this ratio. Omitting this term the weight of resistance for locomotives is 0.005, and for carriages 0.0025. The latter value agrees well with the author's experiments and with an earlier formula by Harding. The former

is much higher than the author's value, who found that a passenger-engine would begin to move on a slope of 0.32 per 100 and a goods-engine on one at 0.39 per 100. To show the weakness of these formulas the writer takes an actual example, in which the value of the resistance was only 652 kilograms, whilst Von Rockl's formulas would give 3,752 kilograms.

The mode of conducting the experiments must therefore be considered. Guillemin employed two methods. In the first, the engine or carriage was brought up to a given speed, and then allowed to run till it stopped. The resistance was then calculated from the following formula:—

$$(M+m)\frac{V^2}{2}=W\,s,$$

where M is the mass of the train, m a quantity depending on the mass of the wheels, V the initial speed, s the distance run, and W the mean resistance. This method, however, was found to produce great discrepancies and difficulties, and was soon abandoned. The other method was to use a dynamometer placed between the tender and the train behind it; but this is defective, because the pressure of the air on the hauling engine is not taken into account, and because at high speeds the vibrations of the dynamometer make its readings inaccurate. Von Rockl's experiments were specially devoted to curves, and were made upon six different curves, laid near the central station at Munich. Beside each line was an electric wire, with a current breaker at every 20 meters, connected with a chronograph. The train was launched upon the curve at a known speed, and the time of passing each successive 20 meters was taken. Such a method may give results fairly applicable to cases of shunting, but not to the ordinary running of trains as hauled by locomotives; and is also subject to the difficulties which attended Guillemin's first method. The author's method was to place the locomotive or train on a straight incline of 1 in 200, and allow it to run down from a fixed initial velocity. The weight of the train then acted to accelerate, and the friction, air-resistance, &c., to retard the speed; which therefore increased up to a certain limit and then became constant. This constant speed was about 13.5 meters per second for trains consisting of a locomotive with tender and 5 to 7 wagons; 10.8 for passenger-locomotives alone; and 8.5 for goods-locomotives alone. With goods-trains it varied according to the number and weight of the wagons, but was not allowed to exceed 12.5 meters per second. The experiments were made on an incline of about 9,000 meters long, with only a short horizontal length in the middle. It is evident that, when at the constant speed, the resistance is equal to the component of the weight, the train not being under steam. This resistance can thus be easily calculated, and inserted in the equation found above, viz.—

$$W=m\,Q+B\,v^2.$$

Since W and Q are known, only one of the values m and B is required to be known in order to determine the other.

A mathematical investigation is given, by means of which the problem can be fully worked out, if the train moves upon a straight incline. To this must be added the effect due to curves, where such occurs, and for this purpose the writer uses the coefficient given above, as determined by Von Röckl from more than 2,000 experiments. The mode of introducing this coefficient into the equation is investigated, and then is found a complete practical formula, which can be applied to the author's experiments. The actual mode of taking these was to bring the engine or train up to the proper speed by its own steam, and at about 100 meters before the first point of observation, to shut off the steam and throw the lever to mid-gear. The time of traversing each 500 meters was then carefully noted, and the mean speed taken therefrom. The largest number of experiments was made with a passenger-engine "Fuse." The earlier ones were made when the engine had been some time at work. It was then taken into the shops and the bearings changed. The result was to produce a considerable increase in the resistance, and diminution in the constant speed generally attained. This speed, however, was found to increase steadily in subsequent experiments, until it nearly reached its original value. Tables and diagrams with this and with other engines and trains are given, and also full particulars of the engines themselves. The results on the whole were uniform, but with some variations, arising

mainly from variations in the wind. In order to compare these results with the author's formulas, an average experiment was taken, and the curves of speed calculated according to the author's formulas, using the known elements of weight, velocity, &c. These curves are found to agree very closely with those plotted from the experimental results.

The mode of working out mathematically, from the results of the experiments, the values of the coefficient, m, is then given at length. On the whole it appears that for four-coupled passenger-engines $m=0.0032$; for six-coupled goods-engines $m=0.0038$; and for carriages and wagons $m=0.0025$. These values are to be used in the following equation, which represents the resistance of a train in the most general manner.

$$W=Q_1 m_1+Q_2 m_2 \pm (Q_1+Q_2)$$
$$\sin.\, a+(Q_1+Q_2)\,\frac{0.6504}{R-55}+l\,(F_1+F_2)\,v^2.$$

Here Q_1 is the weight of the engine and tender, Q_2 the weight of the train, a the angle of inclination of the track, R the radius of the curve, v the speed, m_1 m_2 the coefficients of resistance for the engine or train, l a coefficient for wind-resistance (which may be taken as 0.1225), F_1 the end section of the train (which may be taken at 8 square meters), and F_2 the whole surface exposed to the wind, by all the vehicles of the train together. This latter figure depends of course upon the size and character of each vehicle, and upon the extent to which it is sheltered by the vehicle in front of it. It can only be calculated empirically, and the writer gives the following figures as the allowance of surface he has found best to correspond with his results.

	Square Meter.
For luggage vans	1.7
" carriages and covered goods wagons.	0.5
" open wagons, loaded	0.4
" " empty	1.0
" carriages or covered wagons following an open wagon	1.0

All these quantities are expressed in meters or kilograms, and it is assumed that the lubrication throughout is with oil. To test this equation the author applies it to several of the experiments recorded by Guillemin, and finds the correspondence to be exceedingly close.

The writer was also employed by the railways of Alsace-Lorraine to make a series of experiments, with the view of determining the consumption of fuel on their locomotives. For this purpose it was resolved to ascertain the amount of water evaporated per kilogram of coal, and then to measure accurately the water used both with locomotives alone and complete trains. For this purpose gauge-glasses were fixed on each side of the locomotive and of the tender, and so arranged that the water-level could be accurately taken, and thus the consumption determined. The waste water from the injector was also collected and measured. As the water evaporated represents a definite quantity of work done, this gave an opportunity of comparing the effective work of engines under various circumstances in comparison with the water used.

The resistances to an engine when running without steam have been considered elsewhere. When the engine is under steam, these are increased by the facts that the slide-valves work under the steam-pressure, and that this pressure is also brought upon the eccentric straps, connnecting-rods, ends, &c. Let p be the mean pressure in the valve-chest, o the area of the valve, and f the coefficient of friction, then the friction of the valve is $p\,o\,f$. Let s be the travel of the valve per revolution, and D the diameter of the driving-wheel; then the work done on the two valves per meter run is given by

$$S_1=\frac{2\,p\,o\,f\,s}{D\pi}.$$

The thrust $p\,o\,f$ needed to move the valves is taken by the two eccentric-straps. Let E be the diameter of these, and f_1 the coefficient of friction; then the work done on friction in the two eccentric-straps per meter run is given by—

$$S_2=\frac{2\,p\,o\,f\,f_1\,E}{D}.$$

With regard to the driving gear, there has to be considered the friction between the cross-head and the slide-bars between the cross-head and the connecting-rod, between the connecting rod and the crank-pin, and between the coupling-rods and their pins. Let r be the radius of the crank, l the length of the piston-rod, d_1 the diameter of the cross-head journal, d_2 that of the connecting-rod bearing, and d_3

that of the coupling-rod bearing, f the coefficient of friction for longitudinal motion, f_1 ditto for rotary motion, P the mean pressure on the piston. Then there result the following equations for the work done per revolution of the driving-wheel:—

Friction between cross-head and slide-bars, $$L_1 = \frac{f P r^2 \pi}{l};$$

Friction between cross-head and connecting-rod, $L_2 = 2 f P d_1 \frac{r}{l}$;

Friction between connecting-rod and crank-pin, $L_3 = f_1 P d_2 \pi$.

For the friction of the coupling-rods distinction must be made between four-coupled and six-coupled engines. In the first, half the pressure, or $\frac{P}{2}$, comes on each coupled axle; and therefore,

$$L_4 = f_1 P d_3 \pi.$$

In the second, one-third of the pressure only comes on each coupled axle, and the sum of these pressures on the driving-axle. If d_4 is the diameter of the bearing for the latter,

$$L_4 = \tfrac{2}{3} f_1 P \pi (d_3 + d_4).$$

Dividing the sum of these four values (L_1 L_2 L_3 L_4) by the total work, $L = 4 r P$, the work done in friction can be compared with the whole work done on the engine. The various symbols used must of course have the values belonging to the special engine considered. For a particular six-coupled goods-engine, the writer calculates that the work thus done on friction due to the steam, is only about 4 per cent. Adding this to the work required for overcoming other resistances, as described in the former abstract, the author obtains a general expression for the work done by an engine in taking a train between any two stations at a given speed, over any given curves, and with any number of stoppages, &c. Numerous experiments were made, both with goods and passenger-engines, to test this expression; the coefficient of resistance m having been determined as in the former abstract. The length of the run and the weight of water evaporated were always taken, and thus the work done per kilogram of water could be determined.

Some of the results are given in the following Table:

TABLE OF RESULTS.

Number of Experiments.	Kind of Engine.	Vehicles in Trains, as under—					Water Expended.	Average Speed.	Work Done.	Work per Kilogram of Water.
		Carriages.	Van.	Covered Wagons.	Empty Trucks.	Loaded Trucks.				
	Passenger..						Kilograms per sec.	Meters per sec.	H.P.	Kilogram-meters.
1	"	—	—	—	—	—	0.399	7.06	80.5	15,166
2	"	—	—	—	—	—	0.406	6.76	85.4	15,745
3	"	—	—	—	—	—	0.244	10.83	44.0	13,547
4	"	—	—	—	—	—	0.273	10.50	55.5	15,245
5	"	—	—	—	—	—	0.278	11.48	57.6	15,525
7	"	—	—	—	—	—	0.375	13.20	76.0	15,199
9	"	—	—	—	—	—	0.486	16.60	118.1	18,214
11	"	—	—	—	—	—	0.557	18.20	139.2	18,723
13	"	—	—	—	—	—	0.528	18.20	138.5	19,651
14	"	—	—	—	—	—	0.590	18.40	156.6	19,926
16	"	6	1	—	—	—	0.543	12.03	136.8	18,877
17	"	6	1	—	—	—	0.959	17.07	244.1	19,081
1	Goods.....	—	—	—	—	—	0.280	8.60	46.46	12,420
2	"	—	—	—	—	—	0.274	8.88	57.66	15,786
3	"	—	—	—	—	—	0.287	9.54	51.77	13,519
4	"	—	—	—	—	—	0.341	9.78	65.35	14,382
6	"	—	1	9	19	4	0.491	8.00	171.70	26,253
7	"	—	1	—	—	41	0.560	7.04	179.30	24,014
8	"	—	1	—	—	41	0.566	7.26	189.80	25,126

The experiments embrace as will be seen, very different conditions; the speed with the passenger-engine varying from 7.06 meters per second to 18.4. and the power expended from 44 to 244 H. P.; and in the goods-engine to nearly the same extent.

The three last columns of the tables enable a law to be laid down on the relation of effective work to water expended. Let W be the water expended in kilograms per second, and N the H. P. expended in overcoming resistance. Take the first as ordinates, and the latter as abscisses, and plot the results of the experiments. It will be found that they approximate to two straight lines; that for the passenger-engine corresponding to the equation

$$W=\frac{N}{300}+0.1,$$

and that for the goods-engine to the equation

$$W=\frac{N}{500}+0.18.$$

They give the same water expended for 60 H. P. (viz. 0.3); for greater values of N the goods-engine shows the better results; for less values the passenger-engine.

If x be put for the effective work in kilogrammeters per kilogram of steam evaporated, there is obtained the relation (since 1 H. P. =75 kilogrammeters)

$$x\,W=75\,N.$$

Whence result, using the two formulas above, the two following equations—

For passenger-engines $x=\frac{22{,}500\,N}{N+30}$;

For goods-engines $x=\frac{37{,}500\,N}{N+90}$.

From these formulas may be shown clearly how small is the influence of the speed on the useful effect, and how this effect increases with the work done per second.

Of course at very low speeds the cooling of the cylinder would diminish the useful effect, and at very high speeds the loss of pressure in the cylinder, and the quantity of priming water, would have a similar tendency. But, in practice, these elements are not greater than the ordinary errors of observation. Thus in Nos. 13 and 16 of the Table, where N=138.5 and 136.8, the water expended is 0.528 and 0.543 respectively, or very nearly equal; whilst the speed is 18.2 in the one case, and 12.03 in the other. The above equations may therefore be used quite independently of the speed. With their help it is easy to calculate the work done, and water expended, in sending a known train at a known speed over any particular length of line, on which the curves and inclines are given. Knowing the water expended, the fuel burnt is easily obtained, since the relation between them remains almost constant as long as the quality of the fuel and the conditions do not change. Elaborate experiments made by Wöhler in 1879, and published in the "Centralblatt für Bauverwaltung," 1882, show that, on an average, the weight of water evaporated is seven times the weight of coal burnt.

From these results, the conclusion is drawn that it is desirable to give to each engine the heaviest train it will draw, not only because it is thereby better employed, but because (as seen by the form of the equations above) the useful effect increases with the work done. Of course the engine must not be overloaded, so as to cause too much priming, &c. Again, a succession of rising and falling gradients have a very small effect on the work done, provided that the speed in descending has not to be checked by brakes. Of course, however, the load to be put on an engine must be calculated with reference to the steepest gradient it has to surmount, and the maximum resistance it may encounter on that gradient. The resistances due to curves and gradients respectively are known, and in each railway a maximum value for the sum of these resistances should be assumed, taking the place of what is now called the ruling gradient.

Again, the result that the useful effect is practically independent of speed has important consequences. As the speed diminishes, the resistance of the air diminishes in a much higher proportion, and the tractive power at the same time increases. Hence, lowering the speed is very effective in overcoming great resistances, although a limit is of course set to this by the limited adhesive force between tires and rails. Taking the coefficient of friction as 1 by 7, the limit of tractive force for the goods-engine in the above experiments was 5,500 kilograms, and for the passenger-engine 3,143 kilograms.

The greatest power of the two engines was, for the former, 25,500 kilogram-meters per second, and for the latter 21,000. Dividing these quantities by the former, the minimum speed for the goods-engine is 4.5 meters per second, and for the passenger-engine, 6.7. This minimum speed being fixed, and the other elements of the question known, it is easy to find an equation for the greatest number of vehicles that can be hauled by a given engine over a given line. The results are worked out in the following Table, in which the gradients are those on a straight road, and must be lowered proportionately if there is a curve at the same place.

Vehicle.	Minimum Speed.	Maximum Gradient.							
		1/500	1/400	1/300	1/200	1/150	1/100	1/80	1/50
	Meters per sec.								
Carriages, express trains.....	14.0	—	—	11.3	8.2	6.1	3.3	2.0	—
" ordinary "	10.0	—	—	22.0	16.3	12.8	8.2	6.1	2.5
Covered goods-wagons, loaded.	4.5	62.7	56.2	48.1	36.5	29.2	20.6	16.6	10.0
" " empty.	4.5	—	—	—	—	67.5	47.8	38.6	23 2
Open wagons, loaded.....	4.5	73.3	65.6	56.1	42.6	34.1	24 1	19.4	11.6
" empty.....	4.5	—	—	—	—	—	69.8	56.6	34.2

In the case of a level road, the same equations give the following values for the numbers of vehicles that can be hauled at given speeds.

—	Speed.	Number of Vehicles.
	Meters per sec.	
Carriages, express.....	20.0	8 0
" ordinary.....	16.0	18.8
Covered wagons, loaded.	7.5	61.7
Open wagons, loaded....	7.5	72.5

These figures agree well with experience, and show how rapidly the admissible load diminishes as the speed and gradients are increased; and also the great importance of noting whether the wagons are loaded or empty. Although taken from particular experiments, the equations are general, and may be used for solving any questions upon the movement of trains over railways.

THE FIGURE OF THE EARTH.

By FRANK C. ROBERTS, C. E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

SECTION I.—HISTORICAL.

THE progress of the science of astronomy is the continued triumph of the powers of the intellect over the first erroneous conceptions of the senses; and its history is so allied to that of the human mind that we cannot help feeling a strong inclination to know at what time and by what people the hypotheses upon which the science is based were first advanced.

The value of an hypothesis is estimated by the number of difficult phenomena it explains. From this standpoint few suppositions can be found to have been more important than that which assigned to the earth its approximate figure and magnitude.

Little is known of the early history of this hypothesis, for it is enveloped in those dark ages of antiquity when the revolutions of empires were imperfectly recorded, not to speak of the calm speculations of quiet and thoughtful men.

To its first inhabitants the earth must have appeared as an extended fixed plane, the extremities of which apparently supported the vast dome of the heavens.

Among the ancients the prevailing opinion was that the surface of the earth was flat, that the visible horizon was the boundary of the earth, and the ocean the boundary of the horizon; that the earth and heavens were the whole visible universe, and that all beneath the earth was Hades.

The progress of knowledge concerning the figure of the earth has from the earliest times been closely connected with the study of astronomy. As in this latter science, first impressions are abandoned, and all conclusions are in striking contradiction to those of superficial observation, so, as man progressed, the earth became divested of its flattened shape and character of fixidity, and was shown to be a globular body turning swiftly upon its own axis, and moving through space with great rapidity.

The idea of the earth being a globe is now so familiar to us that arguments in proof of it are almost unnecessary. Yet familiar as this fact now is, many ages must have elapsed before it was universally received. So difficult was it to conceive how the inhabitants of the opposite hemisphere could exist with their heads downwards, that we find St. Augustine in the 5th century vehemently contending against the possibility of the existence of an antipodes.

Evidently ignorant of many important physical laws, the early suppositions of the ancients regarding the figure of the earth are in a great measure ludicrous. Being but imperfectly acquainted with the science of astronomy, and their observations being rude and inaccurate, they were led to base their theories upon false assumptions. It might be well to except from this sweeping statement the theories of the early Eastern astronomers. The earliest astronomical records that can be conceived of as authentic are found in China, and go back as far as 800 B. C., when we find eclipses observed and registered. This would naturally lead us to conclude that the observers were acquainted with the fact that the sun and heavenly bodies were visible to people towards the east sooner than people towards the west. As this could not occur unless the earth were curved, we may assume with almost a certainty that the globular figure of the earth was knwon in China at least 800 years B. C.

It is to be regretted that we have no record of the speculations of the early inhabitants of the eastern countries upon this subject. Astronomy was evidently cultivated as a science, and whatever may have been their suppositions as to the figure of the earth, we may suppose that at the first they were led to conclude that the heavens were spherical by observing that those stars sufficiently elevated towards the north pole performed their entire revolution around the pole without interruption; from which it might by an easy inference be concluded that the other stars, though concealed from view, pursued their course in the same manner. When once the theory of a revolving heaven was accepted, it would be comparatively an easy matter to conclude that the earth was globular.

The only records of early researches in connection with the figure of the earth come to us from Greece. The discovery that the earth is not a plane is ascribed to Thales, of Miletus, B. C., 640. Anaximander B. C. 570, Anaxagoras B. C. 460, claimed a cylindrical shape for the earth, estimating the height as three times the diameter, the land and water being on the upper base. Plato B. C. 400 called the earth a cube. Aristotle advanced the theory that the earth was spherical.

It may be well to understand at this point that when we speak of the figure of the earth, we mean the figure which would be assumed by the earth were it covered entirely with water, and more specifically water at mean sea level.* As for the inequalities on the surface of the earth owing to mountains and valleys, they are of no moment in the estimation of the general figure, being of much less account with respect to relative proportion than the asperities on the surface of an orange with regard to the orange itself. Upon an artificial globe of 6⅔ feet in diameter, Mount Chimborazo† would be represented by a grain of sand less than 1-20 of an inch in thickness.

As we have previously stated, Aristotle was the first to assume the spherical hypothesis. The active curiosity of man, however, did not rest contented with having assumed that the earth was a

* Scientifically speaking the Figure of the Earth is interpreted as meaning the mean surface of the sea imagined to percolate the continents by canals.

† 21,424 feet above mean tide.

sphere, but proceeded to ascertain the exact dimensions of the planet. In the course of his discussion Aristotle states, as does Archimedes (B. C. 250), that mathematicians estimated the circumference of the earth at 300,000 stadia.

The first approximation to the magnitude of the earth, however inaccurate, must have been at that time a most important addition to the stock of natural knowledge, and, indeed, except with a view to some very refined scientific investigation, the general idea which the ancients had of the magnitude of the earth differs but little from that of the moderns, for we are so incapable of the appreciation of number or magnitude when either exceeds a certain limit, that the difference between their results and ours makes little or no difference in the general idea which we hold as to the size of the earth.

Eratosthenes, B. C. 230, was apparently the first to conceive a method for the deduction of the length of the circumference of the earth. Although his results are probably sadly inaccurate, the method which he adopted is in essence identical with that followed at the present time.

As it is impossible for us to occupy a position from which the earth may be viewed as a whole and compared with some standard of measure, we are compelled to resort to geometrical principles in the determination of its figure and magnitude. The problem is rendered more difficult by the fact of there being no fixed landmarks or standard lines upon the surface of the earth indicating aliquot parts of the earth's circumference.

It therefore becomes necessary to refer our situation on the earth to objects external to our own planet. Such marks are afforded by the heavenly bodies.

By observations of the meridian altitudes of stars, and from their known polar distances, we determine the altitude of the pole-star, which is also the latitude of the place at which the observations are made.

Let us suppose then, that we wish to determine the length, on the surface, of one degree of the earth's circumference. Let us suppose also that we know the distance between two places on the *same* meridian. Then having determined the latitudes of the two places, their difference may be taken as representing the angle at the center of the earth corresponding to the measured distance on the surface. Dividing the distance by the angle we find the length of a meridian arc equivalent to one degree of latitude, and this multiplied by 360 gives us the length of the earth's circumference.

Where local difficulties compel the observers to deviate, in the measurement of the distance, from the line of the meridian, the amount of the deviation must be noted. A very simple calculation will enable us to reduce the measured distance to the corresponding length on the meridian. It seems hardly necessary to add that this measurement must be made with the greatest care and accuracy, for an error in the measured length of one degree is multiplied 360 times in the circumference and nearly 58 times in the deduced radius of the earth.

Such in its simplest form is the geodetic operation called the measurement of an arc of a meridian, and is in essence the method employed by Eratosthenes. He knew that at Syene, in S. Egypt, on the day of the summer solstice at mid-day, objects cast no shadows, whence he concluded that the sun was in the zenith. In Alexandria, at the same period, he observed that the sun made an angle with the vertical of 7° 12′ or $\frac{1}{50}$th of a circumference. Assuming Alexandria to be directly north of Syene,* he concluded the length of the circumference to be 50 times the distance between these two places, or 250,000 stadia. Of course this determination was very imperfect, for with the instruments of his time he was compelled to neglect the diameter of the sun in the determination of declination. This occasioned an error of $\frac{1}{2}$° in the length of the celestial arc at Syene. The measurement of the distance between Syene and Alexandria was probably also very inaccurate.

The next attempt to solve the problem was made by Posidonius, B. C. 90. Instead of using the sun for the determination of the difference of latitude, he found the celestial arc by means of the star Canopus. At Rhodes this star when on the meridian is just visible above the horizon, while at Alexandria its meridian

* The error of this assumption was about three degrees.

altitude is 7° 30′. The distance between the two places being known, he deduced 240,000 stadia as the circumference of the earth.

Ptolemy, an astronomer, A. D. 160, in his treatise on Geography, gives 500 stadia as the length of a degree. This value would give for total length of the circumference 180,000 stadia—a result widely different from any previously deduced.

Unfortunately the degree of approximation attained in these results cannot be known, as we have no value for the length of the *stadium*. It probably had different values dependent upon time and place.

In the year 819 the Caliph Almamoun caused the astronomers of Bagdad to measure an arc of the meridian on the plains of Mesopotamia by means of wooden rods. Authorities differ as to the resulting deduction of the length of a degree; some claiming that, failing in their own observations owing to insurmountable obstacles, the Arabians adopted the result of the Grecian astronomer Ptolemy. Others hold that they found for the length of a degree 56⅔ Arabian miles, or approximately 71 English miles.

From this time until the revival of letters, interest in this subject seems to have disappeared. Speculation was at a standstill for 700 years. Former theories and suppositions were forgotten and lost amid the social storms of the middle ages. Man was again ignorant of the form and dimensions of the planet which had been assigned to him in the immensity of space.

Early in the 15th century the question of the form of the earth began a second time to attract the attention of thoughtful men. The prevalent idea was that the earth was a plane. This time it was not the philosophers but the navigators who looked with doubt upon this supposition. Columbus fearlessly asserted the earth to be globular, and after the voyage of Magellan around the earth, the globular hypothesis was once more accepted. Immediately endeavors were made to determine the size of the earth. In 1525 Fernel made a determination of the length of a degree by deducing the difference of latitude between Paris and Amiens, and measuring the distance by observing the number of revolutions made by his coach wheel in traveling from one place to the other. From these observations he deduced the length of one degree to be 57,050 toises or 364,960 English feet. (The toise—an old French measure—is practically equal to 1.949 meters, or 6.3946 English feet.)

In 1617 Willebrord Snell conceived the idea of the deduction of the length of distances by means of a series of triangles measured from a known base. This was the first instance of the application of the invaluable principle of trigonometrical surveying which, since that time, has become general in all extensive surveys.

Snell measured his base line upon the frozen surface of the meadows between Leyden and Soeterwood. The angles he measured by means of a quadrant of 5½ feet radius. His result for the length of a degree was 55,020 toises, or approximately 66.63 miles.

In 1633 Norwood, in England, adopting a method similar to that of Fernel's deduced 57,424 toises for the length of a degree.

We are now brought down to the time of Picard, whose invaluable adaptation of the telescope to circular instruments for measuring angles marks an era in the progress of geodetic science. Hitherto the measurement of angles was roughly made by the use of sights similar, only much more unreliable, to those used on rifles. Picard first introduced spider lines in the focus of telescopes, whereby a far higher degree of precision in determination of the position of a distant point may be obtained by covering the point in question with the intersection of spider lines, which is so placed as to be exactly in the center line of the telescope. In his determination of the length of a degree he used the trigonometrical method, measuring twice a base line of nearly seven miles in length. His measurement is the first executed with anything like scientific precision. He even calculated the error produced by his instrument being out of the center of his station, and determined his difference of latitude by means of the zenith distance of a star in Cassiopeia measured with a sector. At this time the effect of aberration, refraction and nutation were unknown, nevertheless his result of 57,060 toises, or

nearly 69.76 miles is marvelously near that of later determinations.

The measurement of an arc of the meridian, although the most reliable, is not the only method by which the figure of the earth upon the spherical hypothesis may be deduced. One simple expedient consists in determining the dip or angle of depression of the horizon. Take, for instance, the case of a mountain near the sea coast.. Knowing the height of the mountain above the sea, and the angle of depression from its top to the horizon, we can, by an easy mathematical formula, deduce the following equation in which r is the radius of the earth, h the height of the mountain, and d the distance from the mountain to the horizon:

$$r^2=(r+h)^2-d^2.$$

This principle was applied more than 200 years ago at Mount Edgecombe, and since that time at Ben Nevis.

Or, again, we may by the application of the following proposition form a proportion from which the diameter of the earth may be found—the earth's diameter bears the same proportion to the distance of the visible horizon from the eye as that distance does to the height of the eye above the sea level.

Both these methods, of course, only furnish means of determining the size of the earth with a rough approximation. Refraction bends the visual lines out of the truly rectilinear direction, and, therefore, introduces a serious error in the result.

Up to 1690 astronomers supposed the form of the earth to be nearly that of a perfect sphere, and consequently the *length of degrees in all latitudes precisely equal.*

In 1690-1718, J. and D. Cassini published results showing that although the measures of meridional arcs made in various parts of the globe agreed sufficiently to prove that the supposition of a spherical figure is not very remote from the truth, yet exhibited discordances far greater than could be attributable to errors of observation, and which rendered it evident that the spherical hypothesis was untenable. Immediately upon this discovery, new interest was awakened in the subject. The works of previous scientists upon this subject were carefully examined, and, as a result, it became known that Picard, as early as 1671, in his work on the figure of the earth, mentions a conjecture proposed to the French Academy that, supposing the diurnal motion of the earth, heavy bodies should descend with less force at the equator than at the poles; and that, for the same reason, there should be a variation in the length of the pendulum vibrating seconds in different latitudes, for the time of oscillation of a pendulum of constant length depends upon the intensity of the force of gravity.

In the same year Richer was sent to Cayenne, in equatorial S. A., and was especially charged by the Academy to observe the length of the pendulum vibrating seconds. On his return he stated that the difference between the seconds pendulum at Paris and Cayenne was one line and a quarter, that at Cayenne being the shorter. Moreover, the clock which Richer took to Cayenne, having been adjusted to beat seconds at Paris, retarded two minutes a day at Cayenne, so that no doubt remained of the diminution of the force of gravity at the equator.* This, as it was the first direct proof of the diurnal motion of the earth, was also what led Huygens to suspect that there was a protuberance of the equatorial parts of the earth and a corresponding depression of the poles Cassini had already observed this phenomenon in the figure of Jupiter, which analogy strongly favored the supposition of a similar peculiarity in the shape of the earth.† Since, then, it was evident that the meridian section of the earth was not a circle, what was the next simplest supposition that could be made respecting the nature of the meridian. In the flattening of a round figure at two opposite points, and its protuberance at points rectangularly situated to the former, we recognize the distinguishing feature of the elliptic form. Thus mathematicians, after discarding the spherical hypothesis assumed the meridian to be an ellipse. The geometrical properties of that curve

* See Newton's *Principia*, Book III.

† The difference of the diameters of Jupiter amounts almost to 1-10th. and when we compare the exact measure of this depression, the dimensions of Jupiter and the time of his rotation, with like phenomena connected with the earth, we find for this latter planet a proportional depression of 1-388th, which is very nearly identical with the value deduced from the great French measurement.

enabled them to assign the proportion between the lengths of the axes which would correspond to any proposed rate of variation in its curvature, as well as to fix upon the absolute lengths corresponding to any assigned length of a degree in a given latitude.

Spheroids are generated by the revolution of ellipses about one or another of their axes. Every ellipse has two axes, one passing through the *foci* is called the major axis, while the other—perpendicular to the major axis at its middle point—is called the minor axis. When the ellipse revolves about its major axis it generates what is called an *oblate spheroid*, and when it revolves about its minor axis the figure generated is named a *prolate spheroid*. The *ellipticity* is the amount of variation of the form of the spheroid from a sphere of like content, or the amount of flattening at the poles. This is expressed by dividing the difference of the semi-major and minor axes by semi-major axis.

The *eccentricity* of an ellipse is equal to the distance from the center to one of its foci divided by the semi-major axis.

Huygens was the first person who attempted to determine the figure of the earth by direct calculation, but in his investigation he assumes that the whole of the attractive force resides in the center of the earth, and that its power varies as the square of the distance. This hypothesis, since the discovery of the law of universal gravitation, has been found inadmissible, and therefore his results were largely in error.

In the course of the discussion of Cassini's observation of the variation in length of the second pendulum in different latitudes and consequent diminution of the force of gravity at the equator it was claimed that this diminution might be due to the counteracting effect of the centrifugal force occasioned by the rotation of the earth. Newton* showed that even after making allowances for this effect, the difference between the force of gravity at Paris and Cayenne was too great for the spherical hypothesis, and further, upon the assumption that the earth is a homogeneous fluid, and supposing its density to be the same throughout the whole mass, and assuming that the constituent molecules attract one another in proportion to the inverse square of the distance, he demonstrated that, in consequence of rotation, the earth would assume the form of an *oblate spheroid*, whose ellipticity would amount to $\frac{1}{230}$th.

Clairaut was the first to advance a general solution of this problem adapted to the hypothesis of a variable density. He proved that, if the density of the strata of which the earth is composed increases towards the center, the ellipticity will be less than in the hypothesis of Newton, and greater than in that of Huygens; and, again, that the sum of the fraction representing the ellipticity and the fraction expressing the augmentation of gravity at the poles will always make a constant quantity, which is equal to $\frac{5}{2}$ of the fraction which expresses the proportion which exists between the centrifugal force and gravity at the equator. It is by means of this theorem we are enabled to ascertain the figure of the earth by means of pendulum experiments.

These theoretical determinations of Huygens, Newton, and Clairaut were, upon the completion of surveys made by Cassini in France, found to be at variance with his results. He found the length of one degree of a meridian south of Paris to be 57.092 toises, while north of the city it was only 56.960 toises. This led to the conclusion that the earth is a *prolate* spheroid. Here, of course was material for a controversy; in view of this fact the French Academy sent out two expeditions to make measurements that would definitely settle the matter. These expeditions set out in 1735; Bouguer, Godin and La Condamine proceeded to Peru, and after ten years' work they measured an arc of above 3° between the parallels 2′ 31″ N., and 3° 4′ 32″ S. latitude. Maupertius, Clairaut, Camas and Le Monnier, arriving in Lapland, measured an arc of 57 minutes, and returned within 16 months. The results deduced from these observations concurred in proving that the degrees of the meridian increase very sensibly in length from the equator to the high latitudes, and from this time dates the undisputed conclusion that the earth is an *oblate* spheroid, rather than a sphere or prolate spheroid.

The deviation from the spherical form is evidently very slight, the difference be-

* *Principia*, Book III.

tween the equatorial and polar diameter being only 27 miles. As an illustration, on a globe 24 inches in equatorial diameter, and on which the thickness of a sheet of writing paper would represent the elevation of the lands above the waters, the polar axis would be 23.928 inches, or in other words, the difference between the polar and equatorial axes would be but one-fourteenth of an inch. For this reason the spherical hypothesis is sufficiently accurate for many purposes. When this hypothesis is used in geodetical operations the radius of the earth as a sphere is taken as the average of all the radii of the spheroid. This radius is equal to 6.370 kilometers, or 3.958 miles. In the determination of the mean length of an arc of 1°, $\frac{1}{90}$ of the length of an elliptical quadrant of the spheroid is taken. Various values for this quadrant have been computed by different mathematicians. The one deduced by Bessel has been in long use for geodetical computations, and is very nearly the mean of the values found by other investigators. The mean length of one degree is, according to Bessel, 111.121 meters, or 69.043 miles.

It is thus seen that when the spherical hypothesis is applied the assumed sphere is one having an equal volume with that of the oblate spheroid.

There is, however, in these values, a serious inconsistency, for the quadrant of a circle corresponding to the above mean radius is nearly 6 kilometers greater than Bessel's value used in the above determination of the length of 1°. For this reason the value sometimes used for the radius is that of a circle whose circumference is equal to the circumference of a meridian ellipse, or 3.956 miles=6.367 kilometers. This value is 3 kilometers too small, but the error is unavoidable.

That the science of the mathematicians had described in a general way the figures of our globe, was sufficient to satisfy the curiosity of the ordinary individual, but not the zeal of scientists for exact knowledge; they further endeavored to obtain the precise amount of the depression at the poles, whose existence had been proven by so many experiments. Material was accumulated, new arcs were measured, but the difficulty of an exact determination only increased. The different measures of degree lengths gave varying values for this depression upon the oblate-spheroidal hypothesis. An Italian mathematician, named Frisi, showed the variation of the calculated depression very clearly by a comparison of the measures then known. The following is a list of the arcs used by him in his computations, and also of the astronomers to whom we are indebted for their determination:

Country.	Latitude where the measurement commenced.	Value of the degree measured.	Observers.
Peru	$0^d - 0^m$	56.753 tois.	Bouguer, La Condamine, etc.
Cape of Good Hope	33–18	57.107 "	Lacaille.
Pennsylvania	39–12	56.888 "	Mason and Dixon.
Ecclesiastical State	43–01	56.979 "	Boscovich and Maire.
France	43–31	57.048 "	Cassini and Lacaille.
Piedmont	44–44	57.137 "	Beccaria.
France	45–45	57.050 "	Cassini and Lacaille.
Hungary	45–57	56.881 "	Liesganig.
Austria	48–43	57.086 "	"
France	49–23	57.074 "	Picard & Cassini.
Holland	52–04	57.145 "	De Thury & G. Cassini.
Lapland	66–20	57.405 "	Maupertius.

Frisi, in his calculations, sought to determine, according to Newton's theory, the data for a regular curve from which could be derived the above values. In this he was unsuccessful. The curves were either too large or too small.

The values for the ellipticity of the earth's meridian deduced from the sur-

veys instituted by the French Academy are as follows:

Lapland and French arcs, $\frac{1}{144}$th;
Lapland and Peruvian arcs, $\frac{1}{313}$th;
French and Peruvian arcs, $\frac{1}{334}$th.

There was evidently a serious discrepancy either in the assumption as to the form of the earth, or in the accuracy of the determinations, for if the earth were a spheroid of revolution these results should be identical. Following this discovery numerous measurements of arcs of meridian were made in different parts of the world. The most important of these, however, were executed under the direction of the French Government in the determination of the length of the meter—taken as one ten-millionth part of the quadrant of the earth's meridian. These latter observations when combined with the corresponding values in the Peruvian arc gave for the ellipticity, $\frac{1}{334}$th. The nearest approximation of the calculated curve to an ellipse whose minor axis would be to its major in the ratio of 230 to 231 involved an error of more than 100 toises to the degree. Frisi then determined the mean value for the various depressions resulting from the above data and found, for the mean term, a depression almost identical with that furnished by the observations of the pendulum and the measurements for the determination of the French measures.

The evident impossibility of finding a regular curve to correspond to the different degrees measured gave rise to doubts as to the possibility of measuring a degree of the meridian with accuracy. The instruments then employed in the determinations were liable to errors of three or four seconds for the celestial arc, or 60 toises for a terrestrial degree.*

The attraction of mountains upon the plumb line, causing a deviation of the vertical, was another source of error. Thus, if the direction of the plumb line at the extremities of the arc measured deviated from the normal by 15 sec., it would cause an error of 500 toises or 533 fathoms in the final result, a quantity greater than the presumed difference of the two extreme degrees under the equator and the pole.† Towards the end of the last century various attempts were made to reconcile the accumulating data with the spheroidal hypothesis. Among the most prominent investigations are those of Boscovich in 1760, and Laplace in 1793 and 1799. Laplace took, as the basis of his combinations, nine of the measurements used by Frisi. The curve which he calculated gave for the length of a degree a value too small by 137.7 toises (=nearly 268 meters), or approximately nine seconds of latitude. These errors, says Laplace, are too great to be admitted, and it must be concluded that the earth deviates materially from the elliptical figure.*

When compared with the great size of the earth, this deviation of the figure of the earth from the oblate spheroidal form is very slight. As previously stated, for many practical problems it is sufficiently accurate to consider the earth as a sphere, but where, for the purposes of science, it is necessary to apply the spheroidal hypothesis, mathematicians have deemed it expedient to determine the elements of an ellipse agreeing as nearly as possible with the actual meridian section of the earth, and to base their calculations upon the resulting spheroid.

In 1805 Legendre announced the method of least squares for the adjustment of observations, and during the present century numerous applications of this principle in the determination of the mean ellipse of the earth's meridian have been made, the principal of which are given in the table on page 236.†

Of these, the values of Bessel and Clarke are considered the most reliable, and the spheroid deduced from the elements calculated by these investigators are called respectively the Bessel and Clarke spheroids. The dimensions of the terrestrial spheroid deduced by Bessel are as follows:

Greater or equatorial diameter, 7925.604 miles.
Lesser, or polar diameter, 7899.114 miles.
Difference of diameters, or polar compression, 26.491 miles.
Proportion of diameters as 299.15 to 298.15.

Probably the value for the ellipticity deduced from pendulum experiments is nearer the truth than any deduced from geodetic data. The latter values have

* Bouguer, Fig. de la Terre, sect. 1, § 4.
† Malt-Brun. p. 25.

* Hist. Acad. Paris, 1789.
† Jordan.

Year.	By whom.	Ellipticity.	Quadrant in meters.
1819	Walbeck	1 : 302.8	10.000.268
1830	Schmidt	1 : 297.5	10.000.075
1830	Airy	1 : 299.3	10.000.976
1841	Bessel	1 : 299.2	10.000.856
1856	Clarke	1 : 298.1	10.001.515
1863	Pratt	1 : 295.3	10.001.924
1866	Clarke	1 : 295.	10.001.887
1868	Fischer	1 : 288.5	10.001.714
1872	Listing	1 : 289.	10.000.218
1878	Jordan	1 : 286.5	10.000.681
1880	Clarke	1 : 293.5	10.001.869

been continually approaching those of the former, and we have every reason to believe that when perfection of geodetic operations is more nearly approached, the results will be practically identical. We give below the elements of the earth's figure deduced from pendulum observations:

Ellipticity $= \frac{1}{288.5}$. Eccentricity $= \frac{1}{12}$th. Quadrant of E's Meridian section = 10001 kilometers, or 6214.62 statute miles.

In 1859 Gen. de Schubert, in attempting to find a continuous curve for the meridian which would satisfy all measured geodetic arcs, suggested the hypothesis of an elliptic equator and an ellipsoidal figure. The ellipsoid is not a figure of revolution. The meridian sections, as in the spheroid, are ellipses, but the equator, instead of being a circle, is an ellipse. The curves of latitude, however, except the equator, are not plane curves, and consequently not true parallels.

Thus, we see that the ellipsoid has *three* unequal axes at right angles to each other.

Gen. de Schubert embodied his idea in his "Essai d'une Determination de la veritable Figure de la Terre," and deduced from eight meridian arcs an ellipsoid of the following elements:*

$a_1 = 6{,}378{,}566$ metres
$a_2 = 6{,}377{,}837$ "
$b = 6{,}356{,}719$ "

$$f_1 = \frac{1}{292.1} \quad f_2 = \frac{1}{302}. \quad F = \frac{1}{8881}$$

where a_1 a_2 are the semi-equat. axes, b = semi-polar axis, f_1 f_2 = the ellipticities of the greatest and least meridian ellipses, F = the ellipticity of the equator.

In 1860 and 1866 similar calculations were made by Capt. A. R. Clarke. Subsequent investigations led Clarke in 1878 to publish the results of a third discussion, giving as the elements of the ellipsoid the following:

$a_1 = 20{,}926{,}629$ feet.
$a_2 = 20{,}925{,}105$ "
$b = 20{,}854{,}477$ "

$$f_1 = \frac{1}{290}, \quad f_2 = \frac{1}{296.3}. \quad F = \frac{1}{13706}$$

The present opinion in regard to the ellipsoidal hypothesis is that until data of a more general and accurate kind have been accumulated, the elements of a satisfactory ellipsoid cannot be computed. Arcs of longitude are needed, for the ellipticities of the meridians differ by such small quantities, that measurements in their directions alone, are insufficient to determine with much precision the form of the equator and parallels.

Aside from this, the physical improbabilities of an ellipsoidal figure are so great that it seems more reasonable to attribute any apparent departure from the spheroidal figure to effects of *local attraction*. Again, there are physical reasons for supposing a spheroidal earth, but the existence of a fluid ellipsoid can only be explained by supposing the existence of an ellipsoidal nucleus, which all speculators in cosmogony agree in regarding as highly improbable. Arch. Pratt remarks, concerning the ellipsoidal figure, that "if a very large number of arcs in all parts of the world were measured, and local attraction being taken into account, the result gave an ellipsoid with its two equatorial axes differing by a

* Mem. d l'Acad. Imp. des Sciences de St. Petersbourg, VII. Serie, Tome 1, No. 6.

quantity, important when compared with the residual errors of observation, there might be some argument for an ellipsoidal figure."

During the present century the extensive trigonometrical surveys undertaken by many countries have been the means of furnishing a number of long and accurately measured arcs. Of these the most important are the Anglo-Gallic, the Russian, the Indian, and the U. S. Coast Survey. The first three have been used in most of the later determinations of the mean ellipse mentioned in the table on page 236.

Now, it is highly satisfactory to find that the oblate spheroidal figure, thus practically proved to exist, is what theoretically ought to result from the rotation of the earth on its axis. The form of the earth is owing to the reciprocal attractions of its component particles. When a weight is whirled around, it acquires a tendency to recede from the center of its motion, as a stone whirled around in a sling. This tendency is called *centrifugal* force. Supposing the rotation of the earth, a centrifugal force is generated whose general tendency will be to cause objects to fly off the surface. This force diminishes the gravity of particles, and hence they recede from the axis of the earth until, by their number and attraction, they counterbalance the centrifugal force. This is confirmed by experience. There is an actual difference in the force of gravity or downward tendency of the same body when conveyed successively to points in different latitudes. Delicate experiments, conducted with the greatest care, have fully demonstrated the fact of a regular and progressive increase in the weight of bodies, corresponding to the increase of latitude.

Now, let us suppose a globe of the size of the earth to be uniformly covered with water. So long as the body remained fixed the surface of the water would outline a perfect sphere. Immediately attending the introduction of rotation on its axis, a centrifugal force would be developed which would act upon every particle in such a way as to tend to cause it to recede from the axis of rotation. But now every particle would be subject to the action of two forces—the one just mentioned, and that of gravity. The direction of the former would be perpendicular to the axis of rotation, and that of the latter perpendicular to the surface of the water. Since at no position but the line of the equator are these forces directly opposite, they combine to form a third force which urges every particle not situated in the equator towards it with a force dependent upon the velocity of rotation. This latter force and the figure of the resulting surface of the water are so connected that an increase of centrifugal force is always counterbalanced by a proportionate change in the direction of gravity. Therefore the water would recede from the poles and heap itself on the equator. This would leave the polar regions, in the case of the earth, protuberant masses of land. Now, the sea is constantly washing and grinding away the land, and carrying and depositing pebbles and fragments over its bed. Thus, in the case considered, the water beating the polar continents would gradually wear them down, and, as with the molecules of water, so, in turn, the worn-off particles and fragments of the polar land would would be forced towards the equator, till the earth would assume by degrees the form we have shown it to approximate—the oblate spheroid.

Section II.

It is not our purpose to enter into the mathematical discussion of the fluid theory of the figure of the earth, but simply to place before our readers such principles of the spheroidal hypothesis as will lead to practical results. For a complete investigation of the form assumed by a revolving fluid on the principle of gravitation, we would refer our readers to section 2 of Mr. Airy's essay upon the Figure of the Earth contained in the "Encyclopedia Metropolitana."

We have stated that if the earth be considered a fluid mass, the form of the surface will be an oblate spheroid of small ellipticity. Further, its axis will coincide with the axis of revolution, and the surface will everywhere be perpendicular to the direction of gravity. It follows also upon the assumption that the density of the strata varies according to a certain probable law, that the ellipticity is $\frac{1}{229}$.

Let us assume, then, that the mean

figure of the earth is an oblate spheroid, and endeavor to show by what methods an ellipse can be found cutting the plumb line at right angles and with its minor axis coinciding with the axis of the earth. This end may be reached by four methods, and first we will consider how the figure of the earth may be determined from geodetic operations.

Chapter I.

The figure of the earth determined by geodetic operations.

The first step in this method is to measure as accurately as possible a base line of any convenient length, not less than 5 or 6 miles, and as near as possible to the meridian upon which we are to base our calculations. From the extremities of this line the angles are measured between the base line and visual lines joining distant points, also taken as near the meridian as convenient. Knowing the length of one side, and two of the angles of a triangle, we can, by trigonometrical formulae deduce the lengths of the other sides. Repeating the operation with the sides already calculated, and selecting new points to suit the emergencies of the case, we establish a connection between the original base line and a second base at the termination of the chain of triangles, and obtain the length of this second base by calculation. It is then measured, and by a comparison of the calculated and measured results the correctness of the operations is tested. This having been satisfactorily performed, the projections of the sides of the triangles upon the meridian are found, and their sum gives the length of the meridian arc between its two extremities. The latitudes of these two extremities are then observed with great care, and from these data the ellipse—of which the arc is part—is found as follows:

Let l and l' be the latitudes of the extremities of the arc.

" m be the mean of l and l', or the middle latitude.

" λ be the amplitude, or $l-l'$.

" a and b be the semi-axes.

" ε be the ellipticity.

" s be the length of the arc.

" r be the radius vector.

Let θ be the angle r makes with the major axis.

Then

$$\frac{1}{r^2}=\frac{\cos.^2\theta}{a^2}+\frac{\sin.^2\theta}{b^2},\ \text{tan.}\ l=\frac{a^2}{b^2}\ \text{tan.}\ \theta$$

$$\therefore\ \frac{1}{r^2}=\frac{a^2\cos.^2 l+b^2\sin.^2 l}{a^4\cos.^2 l+b^4\sin.^2 l}$$

placing $b=a\,(1-\varepsilon)$

$r=a\,(1-\varepsilon\sin.^2 l)$ neglecting ε^2,

$$\frac{dr}{dl}=-2a\varepsilon\ \sin.l\ \cos.l,\ \frac{d\theta}{dl}=1-2\varepsilon+4\varepsilon\sin.^2 l$$

$$\therefore\ \frac{ds}{dl}=\sqrt{r^2\frac{d\theta^2}{dl^2}+\frac{dr^2}{dl^2}}=a(1-2\varepsilon+3\varepsilon\ \sin.^2 l)$$

$$=a\,(1-\tfrac{1}{2}\varepsilon-\tfrac{3}{2}\varepsilon\cos.\ 2l)$$

$$\therefore s=a[(1-\tfrac{1}{2}\varepsilon)(l-l')-\tfrac{3}{4}\varepsilon(\sin.2l-\sin.2l')]$$

$$=\tfrac{1}{2}\,(a+b)\,\lambda-\tfrac{3}{2}\,(a-b)\sin.\lambda\ \cos.\ 2m. \quad (1)$$

If λ be small, not exceeding 12°, we may place sin. $\lambda=\lambda$, then

$$\frac{s}{\lambda}=\frac{a+b}{2}-3\frac{a-b}{2}\ \cos.\ 2m$$

$$\therefore\ \frac{s'}{\lambda'}=\frac{a+b}{2}-3\frac{a-b}{2}\ \cos.\ 2m'$$

when the lengths, amplitudes and latitudes of two arcs are known.

$$\therefore\ \frac{a-b}{2}=\tfrac{1}{3}\frac{\frac{s}{\lambda}-\frac{s'}{\lambda'}}{\cos.\ 2\,m'-\cos.2\,m},\ \text{and}\quad (2)$$

$$\frac{a+b}{2}=\frac{\frac{s}{\lambda}\cos.2m'-\frac{s'}{\lambda'}\ \cos.2\,m}{\cos.2\,m'-\cos.2\,m} \quad (3)$$

From these formulas the semi-axes, a and b, may be computed and the value for ε, the ellipticity follows.

If we were to substitute in these equations observed values resulting from the various measurements of meridian arcs in different parts of the earth, it would be found that the calculated semi-axes and ellipticities in the various cases would be different. If the figure of the earth were truly spheroidal, and there were no errors in the data, *i. e.*, in the observed amplitudes and measured arcs, the results would come out in complete accordance with each other. But this is evidently not the case.

Let us inquire, then, as to the source of these variations in the calculated spheroid, and in attaining this end we will first

state the assumptions we have made in deducing the above formulae. These are (1) that the meridian arc is an ellipse in accordance with the fluid theory. (2) That the plumb line at all stations is normal to this ellipse.

It is plain that the conclusions of the fluid theory are not applicable to the earth in its present state. The irregularities in the external form of the earth are considerable when theoretically considered, and, of necessity, these irregularities of the surface must be accompanied by irregularities in the mathematical figure of the earth. The height of mountains and depth of seas are, in some places, equal to $\frac{1}{7}$ the difference between the equatorial and semi-polar axes, and we are, therefore, prepared to admit that the surface of the earth is not one of revolution. Nevertheless, there must be some spheroid that agrees very closely with the mathematical figure of the earth and having the same axis of rotation.

The errors due to local attraction result from the deflection of the plumb line from the normal to the assumed ellipse. This is sufficient in certain parts to produce material errors in the vertical, and therefore in the difference of latitude determined by the zenith distances of stars.

Thus, suppose A and B to be two stations, and that at A there is a disturbing force drawing the plumb line through an angle δ; then it is evident that the apparent zenith of A will in reality be that of some other place A′, whose distance from A is $r\delta$, when r = earth's radius. Similarly, if there is a disturbance at B of the amount δ', the apparent zenith of B will be that of some other place B′, whose distance from B is $r\delta'$.

Mountain masses, oceans, and variations in the density of the interior of the earth are the chief causes of *local attraction.*

It is then evident that our assumptions in the preceding formula are at fault, and it is now necessary to discover some means whereby the resulting errors may be eliminated.

Bessel was the first to devise a method whereby the results of all the surveys in the different parts of the earth might be brought to bear simultaneously upon this problem. He was followed by Capt. A. R. Clarke, who discussed the problem at the end of the Ordnance Survey Volume.

Bessel's method is in essence as follows: *Corrections* expressed in algebraic terms are applied to the latitudes of the several stations, dividing the arcs into the subordinate parts, such as to make their measured lengths exactly fit an ellipse. The values of the axes of this ellipse are then determined so as to make the sum of the squares of these corrections a minimum, *i. e.*, an ellipse is deduced which most nearly represents the observations.

We will now endeavor to state briefly how this may be done.

Let us first obtain a formula for correcting the amplitude of an arc so as to make its measured length accord with a given ellipse.

We have equation (1),

$$s=\tfrac{1}{2}(a+b)\lambda-\tfrac{3}{2}(a-b)\sin.\lambda\cos.2m.$$

Suppose now that x, x' are small corrections which must be applied to the observed latitudes to make the measured arc fit the ellipse of which a and b are the semi-axes; λ and m when substituted in the above formula will not give the measured value of s. Instead of them

$$\lambda+x'-x \text{ and } 2m+x'+x$$

must be substituted. Hence omitting very small quantities

$$2s=(a+b)\lambda-3(a-b)\sin.\lambda\cos.2m$$
$$+(x'-x)[a+b-3(a-b)\cos.\lambda\cos.2m]$$

$$\therefore x'-x=\left(\frac{2s}{a+b}-\lambda+3\frac{a-b}{a+b}\sin.\lambda\cos.2m\right)$$
$$\left(1+3\frac{a-b}{a+b}\cos.\lambda\cos.2m\right). \quad . \quad (4)$$

Now the mean radius of the earth is known not to differ much from 20890000 feet, and the ellipticity not much from $\frac{1}{300}$. It is therefore convenient to put $a+b$ under the form

$$\frac{a+b}{2}=\left(1-\frac{u}{10000}\right)20890000 \quad . \quad (5)$$

$$\frac{a-b}{2}=\frac{1}{600}\left(1-\frac{u}{10000}+\frac{v}{50}\right)20890000. \quad (6)$$

$$\therefore \varepsilon=\frac{1}{300}\left(1+\frac{v}{50}\right) \quad . \quad . \quad (7)$$

where the squares of u and v may be neglected.

Substituting these in formula (4) and changing the form, we have

$$x' = m + au + \beta v + x,$$

where m, a, β, are functions of the observed latitudes, the measured lengths and other numerical quantities.*

In every measured arc, not only are the extreme stations determined in latitude, but also a number of intermediate stations; one of them should be taken as a station of reference, and each separate error should be written in terms of the reference station error.

Thus if we have five stations,

$$x_1 = x_1$$

$$x_2 = m + au + \beta v + x_1$$

. . . . . .

$$x_5 = m'' '' + au + \beta v + x_1.$$

Now, since these equations are affected by errors of observation, it is impossible to find values of u and v which will satisfy all the equations, and therefore it is necessary to find their most probable values. According to the method of least squares, those values of u and v are the most probable, which render the sum of the squares of all the errors a minimum.

In proceeding with this method, we first deduce normal equations for u by multiplying each observation equation by the coefficient of u in that equation, and add the results; likewise, deduce normal equations for v by multiplying each observation equation by the coefficient of v in that equation, and add the results. Thus we will have two normal equations, each containing two unknown quantities, and the solution of these will give us the most probable values for u and v. Substituting these values in equations (5), (6), (7), we obtain values for the two semi-axes and the ellipticity.

In conclusion, let us examine what effect any error in the amplitudes will have upon the resulting axes.

If we differentiate equations (2, 3), there will appear in the denominators of the resulting expressions the quantity

$$\cos 2m - \cos 2m'.$$

The errors in the axes dependent upon the amplitude errors will therefore be least when this quantity is a maximum. If an arc is located in the southern half of the quadrant, cos 2 m is positive, then

$$2m' = 180°, \text{ or } m' = 90°$$

will give the best result. If another arc is in the northern half, cos $2m$ is negative, and

$$\cos 2m' = 0$$

will give the best result.

Therefore the nearer one arc is to the pole, and the other to the equator, the less will be the effect upon the calculations of any errors in the observed amplitudes.

Chapter II.

THE FIGURE OF THE EARTH DEDUCED FROM PENDULUM EXPERIMENTS.

Upon the hypothesis of the earth being a fluid mass, Clairaut deduced an equation showing that the increase of gravity in passing from the equator to the poles varies as the square of the sine of the latitude, and that a certain relation must necessarily exist between the ellipticity and the force of gravity. Therefore, if we desire to use this theorem in the determination of the figure of the earth, we must first obtain some practical method of measuring the force of gravity in any latitude. The necessity of using the pendulum for this purpose will easily be seen if we consider the impossibility of ascertaining the magnitude of the force by an experiment upon the single descent of free bodies. The quantity to be measured is the velocity which gravity creates in any freely descending body by its action continued during a second of time, and in conducting a series of experiments it is usual to observe the number of vibrations made by the same seconds pendulum in the different places at which it is proposed to compare the force of gravity, and likewise the number of vibrations made at London or Paris. The comparative number of vibrations being found, the comparative force of gravity or the comparative length of the seconds pendulum can be deduced; since the length of the seconds pendulum has been very accurately determined at London and Paris, its length in any latitude may be found.

* The values of m, a and β, have been calculated for the principal arcs and may be found in the British Ordnance Survey volume.

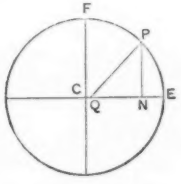
We will now endeavor to show by what means we deduce the Figure of the Earth from Pendulum experiments.

We have for the attraction at any point of a spheriod the following expression:

$$\frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}\left\{1-(2e-m)-\left(\frac{5m}{2}-e\right)\left(\frac{f^2+g^2}{c^2}\right)\right\}. \quad (8)$$

$$m = \text{in the earth} = \frac{1}{289}$$

In which m = ratio of centrifugal force to gravity, f and g = the co-ordinates of the point, in the directions of CD and CE respectively, c = polar axis, e = ellipticity and $\frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}$ = gravity at the equator.



Let EF represent the earth's surface, and let PQ be the normal at P.

Then P Q N is the latitude of P and

$$QN = PQ, \cos. l.$$

Now as we shall have to substitute only in the small terms of the equation, PQ = c nearly, and QN = CN nearly,

$$= \sqrt{(f^2+g^2)} \therefore \sqrt{(f^2+g^2)} = c, \cos. l$$

nearly.

Substituting this in equation (8), we have for the force of gravity

$$= \frac{4\pi}{3}\frac{\Phi(c)}{c^2}\left\{1-(2e-m)-\left(\frac{5m}{2}-e\right)\cos.^2 l\right\}$$

$$= \frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}\cdot\left\{1-\left(e+\frac{3m}{2}\right)+\left(\frac{5m}{2}-e\right)\sin.^2 l\right\}$$

$$= \frac{4\pi}{3}\cdot\frac{\Phi(c)}{c^2}\cdot\left\{1-\left(e+\frac{3m}{2}\right)\right\}\left\{1+\left(\frac{5m}{2}-e\right)\sin.^2 l\right\}$$

Gravity, therefore, may be generally expressed by the formula

$$E(1+n\sin.^2 l), \quad . \quad . \quad . \quad (9)$$

where E = equatorial gravity and

$$n+e=\frac{5m}{2}.$$

Now let p and p' be the lengths of the seconds pendulum in latitudes l and l', P that at the equator, then from equation (9) we have

$$p = P(1+n\sin.^2 l) \quad . \quad . \quad (10)$$
$$p' = P(1+n\sin.^2 l')$$

where

$$n = \tfrac{5}{2}m - e$$

$$\therefore n = \frac{1-\frac{p}{p'}}{\frac{p'}{p}\sin.^2 l' - \sin.^2 l} = \frac{1-\frac{p}{p'}}{\sin.^2 l' - \sin.^2 l}$$

and

$$e = \frac{5m}{2} - n$$

In applying the preceding equations to a series of pendulum observations for the determination of the figure of the earth, the principle of Least Squares is applied as in the last chapter to a number of observation equations of the form (10), thereby obtaining the most probable values for P and n.

Chapter III.

The Ellipticity of the Earth deduced from observed Inequalities of the Moon's Motion.

In the expression for the tangent of the moon's latitude there is this term

$$-\left(E-\frac{m}{2}\right)\cdot\frac{4.(60)^2}{n^2\pi}\text{-sin.}^2\text{ parallax.}\ \frac{E}{\mu}$$

sin. obliquity, cos. obliquity, sin. θ

Now $\frac{E}{\mu}=\frac{\text{Earth's Mass}}{\text{Earth}+\text{Moon}}=\frac{70}{71}$, nearly,

$$n=\frac{27.25}{365.25}$$

The mean horizontal parallax $\frac{E(r)}{M(r)}=57'$, obliquity $= 23°-28'$ nearly,

$$\therefore \text{term} = -\left(e-\frac{m}{2}\right).4891'' \text{ sin. } \theta$$

Now this inequality has been found to exist and its magnitude has been inferred rom observation. It has the effect of fncreasing the apparent inclination of the moon's orbit in one position of her nodes and diminishing it as much in the opposite position.

It is found by observation that the coefficient $= -8''$

$$\therefore \left(E-\frac{m}{2}\right)=\frac{8'}{4891}=.001635$$

and $\frac{m}{2}=.001730$

$$\therefore \varepsilon=.003365=\frac{1}{297}$$

THE EARTH'S ELLIPTICITY DEDUCED FROM THE PRECESSION OF THE EQUINOXES.

The formula expressing the annual Precession is the following:

$$\frac{C-A}{C}\frac{3n'}{n}\cos. I\left(1+\frac{n''^2 1-\frac{3}{2}\sin.^2 i}{n''^2(1+v)}\right)180^\circ$$

in which I= the obliquity of the ecliptic $=23^\circ-28'-18''$, i = inclination of moon's orbit to ecliptic $=5^\circ-8'-50''$, n and n' are the mean motions of the earth around its axis and around the sun and their ratio $=365.26$, n'' the mean motion of the moon around the earth $=27.32$ days, v= ratio of the masses of earth and moon $=75$.

Substituting these quantities

$$\text{Annual Precession} = 16225''.6\frac{C-A}{C}$$

where A and C are the principal moments of inertia of the mass, the latter about the axis of revolution.

* See Pratt's Fig. of the Earth. Page 151.

$$\text{Now } \frac{C-A}{C}=1.98177.(\varepsilon-\tfrac{1}{2}m)'*$$

$\therefore$ Annual Precession $=32155''\ (\varepsilon-\frac{1}{2}m)$

But the Precession by observation $=50''.1$

$$\therefore \varepsilon-\tfrac{1}{2}m=50.1\div 32155=0.0015581$$

$$\therefore \varepsilon=0.0015581+0.0017271=\frac{1}{304}$$

In estimating the reliance to be placed on these results it must be observed that unless the observations extend over a period greater than 20 years they are insufficient. This renders the resulting determinations less reliable than they otherwise would be, as in all probability the observations which are compared have been made by different persons and in different manners. The small lunar inequalities, besides, are involved among a mass of terms greater than themselves. Airy remarks concerning this fact that an error in their determination has less influence on the value of ε than an equal error in the determination of nutation. These facts show clearly that the deduction of the two preceding divisions cannot be compared with those of geodetic measures and pendulum observations. However, the close agreement of the results with those deduced from geodetic measures and pendulum experiments is significant. It shows that in the main the spheroidal hypothesis is correct.

ENGINEERING INVENTIONS SINCE 1862.*

By Sir FREDERICK JOSEPH BRAMWELL, F.R.S.

From "Iron."

I.

Born in the year this institution was founded, and by God's mercy permitted to continue an active worker for more than half a century, I now find myself, by your favor, elected to the presidency. In accordance with established usage I have the privilege, but also, I regret to say, the heavy burden, of delivering an address; a privilege, because, by your courtesy, I see around me an audience of most distinguished men; a heavy burden, because of my inability to make that address worthy of my audience. I can imagine that in many societies, each president, as he takes office, must find an increase in the difficulty of selecting a subject for his address. He must feel that the area to which he is restricted has been so well trodden by his predecessors that it is hardly possible for him to strike out a new path on untraversed ground; but it seems to me that many years must elapse (indeed, I doubt if the time will ever come) when the president of the Institution of Civil Engineers need fear such a condition of things.

* Address of Sir Frederick Joseph Bramwell, F. R. S., on his election as President of the Institution of Civil Engineers, January 13th, 1885.

Our profession is so widespread, it ramifies in so many directions, that successive presidents cannot be embarrassed for want of a subject, but rather must find a difficulty in making a selection among many subjects. I have been determined in my choice by the consideration that our member, His Royal Highness the Prince of Wales, has seen fit to honor the institution by appointing its president to the chairmanship of the executive council of the exhibition of inventions, which is to take place this year at South Kensington. I have, therefore, thought it would be fitting and appropriate that, in my double capacity of your president and chairman of that council, I should direct attention this evening to some of the matters which will be, or which ought to be, contributed to the exhibition. The exhibition will consist of two divisions very dissimilar in their nature, the one "inventions" and the other "music." Division I. (inventions) is separated into thirty one groups, and these groups into 165 classes, but to a few of these only will reference be made. In fact, I must restrict myself to such matters as are more particularly connected with civil engineering, which, as defined by our charter, may be summarized as comprising "all engineering other than military." I must, however, make a still further limitation, for engineering, regarded in this sense, is so comprehensive in its scope, that it is difficult to say what there is of invention, in which the engineer has not an interest. Even a purely chemical invention may not be foreign to our consideration; take, as a test of this, an invention relating to the manufacture of gas, or to the purification of water, or to the treatment of metals, or to some kindred subject.

I propose, therefore, to devote the very limited time at my disposal to the consideration of some of the most important of those improvements which are obviously and immediately connected with civil engineering. I am aware of the danger there is of making a serious mistake, when one excludes any matter which at the moment appears to be of but a trivial character. For who knows how speedily some development may show that the judgment which had guided the selection was entirely erroneous, and that that which had been passed over was, in truth, the germ of a great improvement? Nevertheless, in the interests of time, some risk must be run, and a selection must be made; I propose, therefore, to ask your attention while I consider certain of (following the full title of Division I.) "The apparatus, appliances, processes and products invented or brought into use since 1862." In those matters which may be said to involve the principles of engineering construction, there must, of necessity, be but little progress to note. Principles are generally very soon determined, and progress ensues, not by additions to the principles, but by improvement in the methods of giving to those principles a practical shape, or by combining in one structure principles of construction which had been hitherto used apart. Therefore, to avoid the necessity of having a pause, in referring to a work, by finding that one is overstepping the boundary of principle and trenching within the domain of construction, I think it will be well to treat these two heads together. If my record had gone back to just before 1851 (the date of the great exhibition), I might have described much progress in the principles of girder construction; for, shortly prior to that date, the plain cast-iron beam, with the greater part of the metal in the web, and with but little in the top and bottom flange, was in common use, and even in the preparation of the building for that exhibition, it is recorded that one of the engineers connected therewith had great difficulty in understanding how it was that the form of openwork girder, with double diagonals introduced therein (a form which was for years afterwards known as the exhibition girder) was any stronger than a girder with open panels separated by uprights, and without any diagonals. But, long before 1862, the Warren and other truss girders had come into use, and I am inclined to say, that so far as novelty in the principle of girder construction is concerned, I must confine myself to that combination of principles which is represented by the suspended cantilever, of which the Forth Bridge, only now in course of construction, affords the most notable instance. It is difficult to see how a rigid bridge, with 1,700 feet spans, and with the necessity for so much clear headway below,

could have been constructed without the application of this principle.

Pursuing this subject of bridge work: The St. Louis Bridge of Mr. Eads may, I think, be fairly said to embody a principle of construction novel since 1862, that of employing for the arch ribs tubes composed of steel staves hooped together. Further, in suspension bridges there has been introduced that which I think is fairly entitled to rank among principles of construction, the light upper chain, from which are suspended the linked truss-rods, doing the actual work of supporting the load, the rods being maintained in straight lines, and without the flexure at the joints due to their weight. In the East River Bridge, New York, there was also introduced that which I believe was a novelty in the mode of applying the wire cables. These were not made as untwisted cables and then hoisted into place, thereby imposing severe strains upon many of the wires composing the cable through their flexure over the saddles and elsewhere, but the individual wires were led over from side to side, each one having the length appropriate to its position, and all, therefore, when the bridge was erected, having the same initial strain and the same fair play. Within the period we are considering, the employment of testing machines has come into the daily practice of the engineer; by the use of these he is made experimentally acquainted with the various physical properties of the materials he employs, and is also enabled in the largest of these machines to test the strength and usefulness of these materials, when assembled into forms, to resist strains, as columns or as girders. I, of course, do not for one moment mean to say that experimental machines were unknown or unused prior to 1862—chain-cable testing machines are of old date, and were employed by our past-president, Mr. Barlow, and by others, in their early experiments upon steel—but I speak of it as a matter of congratulation that, in lieu of such machines being used by the few, and at rare intervals upon small specimens, for experimental purposes, they are now employed in daily practice, and on a large scale. In harbor work we have had the principle of construction employed by Mr. Stoney at Dublin, where cement masonry is moulded into the form of the wall for its whole height and thickness, and for such a length forward as can be admitted, having regard to the practical limit of the weight of the block, and then, the block being carried to its place, is lowered on to the bottom which has been prepared to receive it, and is secured to the work already executed by groove and tongue.

It would not be right, even in this brief notice of such a mode of construction, to omit mention of the very carefully thought-out apparatus by which the blocks are raised off the seats whereon they have been made and are transported to their destination. It is no simple undertaking (even in these days) to raise (otherwise than hydraulically) a weight of 350 tons, which is the weight of the blocks with which Mr. Stoney deals. But he does this by means of pulley blocks attached to shears built on the vessel which is to transport the block, and he contrives to lift the weight without putting upon his chains the extra strain due to the friction of the numerous pulleys over which they pass. The height of the lift is only the few inches needed to raise the block clear of the quay on which it has been formed, and this is obtained by winding up the chain by steam gear quite taut, so as to take a considerable strain, but not that equal to the weight of the block, and then water is pumped into the opposite end of the vessel to that upon which the shears are carried, this latter end rises, and the block is raised off the seat on which it was formed without the chains being put to work to do the actual lifting at all. The vessel, with the block suspended to the shear legs and over the bows, is then ready to be removed to the place where the block has to be laid. A word must here be said about an extremely ingenious mode of dealing with the slack chain, to prevent its becoming fouled and not paying out properly when the block is being lowered. This is accomplished by reeving the slack of each chain over two fixed sets of multiple sheaves. A donkey engine works a little crab having a large drum, the chain from which is connected with the main chain, and draws it round the multiple sheaves, so as to take up the slack as fast as the main crab gives it out. The steam is always on the donkey, which is of such limited dimensions

that it can do no injury to the chain even when its full power is in vain endeavoring to draw it any further; directly, however, the main crab gives more slack, and the chain between it and the two sets of sheaves falls into a deeper catenary, and one which, therefore, puts less opposition to the motion of the donkey engine, that engine goes to work and makes a further haul upon the slack, and in this way, and automatically, the slack is kept clear.

A noteworthy instance of the use of pneumatic appliances in cylinder-sinking for foundations, is that in progress at the Forth Bridge. The wrought-iron cylinders are 70 feet in diameter at the cutting edge, and have a taper of about 1 in 46. They are, however, at a height of 1 foot above low water (that is, at the commencement of the masonry work at the pier) reduced to 60 feet in diameter; at their bottoms there is a roofed chamber, into which the air is pumped, and in which the men work when excavating, this roof being supported by ample main and cross-lattice girders. Shafts with air-locks and pipes for admitting water and ejecting silt are provided. The air-locks are fitted with sliding doors, worked by hydraulic rams, or by hand, the doors being interlocked in a manner similar to that in which railway points and signals are interlocked, so that one door cannot be opened until the other is closed. The hoisting of the excavated material is done by a steam engine fixed outside the lock, this engine working a shaft on which there is a drum inside the lock, the shaft passing air-tight through a stuffing box. A separate air-lock, with doors, ladders, &c., complete, is provided to give ingress and egress to the workmen. I have already adverted to one Scotch bridge; I now have to mention another, viz., the Tay Bridge, also now in course of construction. Here the cylinders are sunk, while being guided through wrought-iron pontoons, which are floated to their berths and are then secured at the desired spot by the protrusion, hydraulically of four legs, which bear upon the bottom, and thus, until they are withdrawn, convert the pontoon from a floating into a fixed structure.

I regret that time will not admit of my giving any description of the modes of "cut and cover" which have been proposed for the performance of sub-aqueous works; sometimes the proposition has been to do this by means of cofferdams, and with the work therefore open to the daylight during execution, and sometimes by movable pneumatic appliances. Consideration of sub-aqueous works necessarily leads the mind to appliances for diving, and although its date is considerably anterior to 1862, I feel tempted, as I believe the construction is known to very few of our members, to say a few words about the diving apparatus known as the "Bateau-plongeur," and used at the "barrage" on the Nile. This consists of a barge fitted with an air-tight cabin, provided with an air-lock, and having in the center of its floor a large oval opening, surrounded by a casing standing up above the water-line. In this casing slides another casing telescopically, the upper part of which is connected to the top of the fixed casing by a leather "sleeve." When it is desired to examine the bottom of the river, the telescopic tube is lowered till it touches the bottom, and then air is pumped into the cabin until the pressure is sufficient to drive out the water, and thus to expose the bottom. This appears to be a very convenient arrangement for shallow draughts of water. Reverting for a moment to Mr. Stoney's work, I may mention that he uses for the greater depths he has to deal with, when preparing the bed to receive his blocks, a diving apparatus which (while easily accessible at all times) dispenses with the necessity of raising and lowering needed in an ordinary diving bell, to allow of the entrance and exit of the workmen. Mr. Stoney employs a bell of adequate size, from the summit of which rises a hollow cylinder, furnished at the top with an air-lock, by which access can be obtained to the submerged bell. Beyond the general improvement in detail and in the mode of manufacture, and with the exception of the application of the telephone, there is probably not much to be said in the way of invention or progress in connection with the ordinary dress of the diver. But one great step has been made in the divers' art by the introduction of the chemical system of respiration, the invention of Mr. Fleuss. He has succeeded in devising a

perfectly portable apparatus, containing a chemical filter, by means of which the exhaled breath of the diver is deprived of its carbonic acid; the diver also carries a supply of compressed oxygen, from which to add to the remaining nitrogen oxygen, in substitution for that which has been burnt up in the process of respiration. Armed with this apparatus, a diver is enabled to follow his avocations without any air tube connecting with the surface, indeed, without any connection whatever. A notable instance of a most courageous use of this apparatus was afforded by a diver named Lambert, who, during one of the inundations which occurred in the construction of the Severn tunnel, descended into the heading, and proceeding along it for some 330 yards (with the water standing some 35 feet above him), closed a sluice door, through which the water was entering the excavations, and thus enabled the pumps to unwater the tunnel. Altogether, on this occasion, this man was under the water, and without any communication with those above, for one hour and twenty-five minutes. The apparatus has also proved to be of great utility in cases of explosions in collieries, enabling the wearer to safely penetrate the workings, even when they have been filled with the fatal choke damp, to rescue the injured, or to remove the dead.

With respect to the subject of tunneling, thus incidentally introduced—in subaqueous work of this kind, I have already alluded to that which is done by "cut and cover," but where the influx of water is a source of great difficulty, as it was in the old Thames tunnel (though, in this case, for water one should read silt or mud), I do not know that anything has been devised so ingenious as the Thames tunnel shield; improvement has, however, been made by the application of compressed air. In the instance of the Hudson River tunnel, the work was done in the manner proposed so long ago as the year 1830, by Lord Cochrane (Earl Dundonald) in that specification of his, No. 6,018, wherein he discloses, not merely the crude idea, but the very details needed for compressed-air cylinder-sinking and tunneling, including air-locks and hydraulically-sealed modes, for the introduction and extraction of materials. I may, perhaps, be permitted to mention that some few years ago I devised for a tunnel through the water-bearing chalk a mode of excavation by the use of compressed air to hold back the water, and combined with the employment of a tunneling machine. This work, I regret to say, was not carried out. But there are, happily, cases of sub-aqueous tunneling where the water can be dealt with by ordinary pumping power, more or less extensive, and where the material is capable of being cut by a tunneling machine. This was so in the Mersey tunnel, and would be in the Channel tunnel. In the Mersey tunnel, and in the experimental work of the Channel tunnel, Colonel Beaumont and Major English's tunneling machine has done most admirable work. In the 7-foot 4-inch diameter heading, in the new red sandstone of the Mersey tunnel, a speed of as much as 10 yards forwards in 24 hours has been averaged, while a maximum of $11\frac{3}{8}$ yards has been attained; while, in the 7-foot heading for the channel tunnel, in the grey chalk, a maximum speed of as much as 24 yards forwards in the 24 hours has been attained on the English side, and with the later machine put to work at the French end, a maximum speed of as much as $27\frac{1}{8}$ yards forwards in the 24 hours has been effected. In ordinary land tunneling, since 1862, there has been great progress, by the substitution of dynamite and preparations of a similar nature for gunpowder, and by the improvements in the rock-drills worked by compressed air, which are used in making the holes into which the explosive is charged. For boring for water, and for many other purposes, the diamond drill has proved of great service, and most certainly its advent should be welcomed by the geologist, as it has enabled specimens of the strata passed through to be taken in the natural unbroken condition, exhibiting not only the material and the very structure of the rock, but the direction and the angle of the dip of the beds.

Closely connected with tunneling machines are the machines for "getting" coal. This "getting," when practiced by manual labor, involves, as we know, the conversion into fragments and dust of a very considerable portion of the underside of the seam of coal, the workman

laboring in a confined position, and in peril of the block of coal breaking away and crushing him beneath it. Coal-getting machines, such as those of the late Mr. Firth, worked by compressed air, reduce to a minimum the waste of coal, relieve the workman of a most fatiguing labor in a constrained position, and save him from the danger to which he is exposed in the hand operation. It is a matter of deep regret on many grounds, but especially as showing how little the true principles of political economy are realized by working men, who are usually well informed on many other points, that the commercial failure of these machines is due to their opposition. In connection with colliery work, and indeed in connection with explosives, in the sense of a substitution for them of sources of expansion acting more slowly, mention should be made of the hydraulic wedges. The employment of these in lieu of gunpowder, to force down the block of coal that had been undercut, is one of the means to be looked to for diminishing the explosions in collieries. Another substitute for gunpowder is found in the utilization of the expansion of lime when wetted. This has given birth to the lime cartridge, the merits of which are now universally recognized, but it is feared that trade prejudices may also prevent its introduction. While on this subject of "accidents in mines," it will be well to call attention to the investigations that have been made into the causes of these disasters, and into the probable part played by the minute dust which prevails to so great an extent in dry collieries. The experiments of our honorary member, Sir Frederick Abel, on this point have been of the most striking and conclusive character, and corroborate investigations of the late Maquorn Rankine into the origin of explosions in flour mills and in rice mills, which had previously been so obscure. The name of Mr. Galloway should also be mentioned as one of the earliest workers in this direction. At first sight, pile-driving appears to have but little connection with explosives, but it will be well to notice an invention which has been brought into practical use, although not largely (in this country, at all events), for driving piles, by allowing the monkey to fall on a cartridge placed in a cavity in the cap on top of the pile; the cartridge is exploded by the fall, and in the act of explosion drives down the pile and raises the monkey; during its ascent and before the completion of its descent time is found for the removal of the empty cartridge and the insertion of a new one.

In the days of Brindley and of Smeaton, and of the other fathers of our profession, whose portraits are on these walls, canals and canalized rivers formed the only mode of internal transit, which was less costly than horse traction, and, thanks to their labors, the country has been very well provided with canals; but the introduction of railways proved, in the first instance, a practical bar to the extension of the canal system, and, eventually, a too successful competitor with the canals already made. Frequently the route that had been selected by the canal engineer was found (as was to be expected) a favorable one for the competing railway, and the result was, the towns that had been served by the canal were served by the railway, which was thus in a position to take away even the local traffic of the canal. For some time it looked as though canal and canalized river navigations must come to an end, for although heavy goods could be carried very cheaply on canals, and with respect to the many works and factories erected on the canal banks, or on bases connected therewith, there was with canal navigation no item of expense corresponding to the cost of cartage to the railway stations, yet the smallness of the railway rates for heavy goods, and the greater speed of transit, were found to be more than countervailing advantages. But when private individuals have embarked their capital in an undertaking, they do not calmly see that capital made unproductive, nor do they refrain from efforts to preserve their dividends, and thus canal companies set themselves to work, to add to their position of mere owners of water highways, entitled to take toll for the use of those highways, the function of common carriers, thus putting themselves on a par with the railway companies, who, as no doubt is within the recollection of our older members, were in the outset legalized only as mere owners of iron highways, and as the receivers of toll from any persons

who might choose to run engines and trains thereon—a condition of things which was altered as soon as it was pointed out that it was utterly incompatible, either with punctuality or with safe working. This addition to the legal powers of the canal companies, made by the Acts of 1845 and 1847, has had a very beneficial effect upon the value of their property, and has assisted to preserve a mode of transport competing with that afforded by the railways. Further, the canal proprietors have from time to time endeavored to improve the rate of transport, and with this object have introduced steam in lieu of horse haulage, and by structural improvements have diminished the number of lockages. Many years before the period we are considering, there was employed, to save time in the lockages, and to economize water, the system of inclined planes, where, either water borne in a traveling caisson, as on the Monklands incline, or supported on a cradle, as in the incline at Newark, in the State of New Jersey, the barges were transferred from one level to another; but an important improvement on either of these modes of overcoming a great difference of level is the application of direct vertically lifting hydraulic power. A notable instance of this system was brought before the institution in a paper read on the "Hydraulic Canal Lift at Anderton, on the River Weaver," by S. Duer,* and another instance exists on the Canal de New Fosse, at Fontinettes, in France, the engineers being Messrs. Clark and Standfield, who have other lifts in progress. The system reduces the consumption of water, and the expenditure of time to a minimum.

With respect to canalized rivers, the difficulty that must always have existed when these rivers (as was mostly the case) were provided with weirs to dam up the water for giving power to mills, has been augmented of late years by the change in the character of floods. It has frequently been suggested that in these days of steam motors in lieu of water power, and of railways in lieu of water carriage, the injury done by obstructing the delivery of floods is by no means compensated by the otherwise all but costless power obtained, or by the preservation of a mode of transport competing with railways. It has thereupon been suggested that it would be in the interests of the community to purchase and extinguish both the manufacturing and the navigating rights, so as to enable the weirs to be removed, and free course to be provided for floods. It need hardly be said, however, that if means could be devised for giving full effect to the river channels for flood purposes, while maintaining them for the provision of motive power and of navigation, it is desirable that this should be done. The great step in this direction appears to be the employment of readily, or, it may be, of automatically movable weirs. Two very interesting papers on this subject, by Messrs. Vernon Harcourt and R. B. Buckley, were read and discussed in the session 1879–1880. These dealt, I fear exclusively, with foreign—notably with French and Indian—examples. I say I fear, not in the way of imputing blame to the authors for not having noticed English weirs, but because the absence of such notice amounts to a confession of backwardness in the adoption of remedial measures on English rivers. An instance, however, of improvement since then has been the construction by Mr. Wiswall, the engineer to the Bridgewater Navigation Company (on the Mersey and Irwell section of that navigation), of the movable Throstle Nest weir at Manchester. It does seem to me that, by the adoption of movable weirs, rivers in ordinary times may be dammed up to retain sufficient water to admit of a paying navigation, and water for the mills on their banks, while in time of flood they shall allow channels as efficient for relief as if every weir had been swept away. But the great feature in late years of canal engineering is not the preservation or improvement of the ordinary internal canal, but the provision of canals, such as the completed Suez Canal, the Panama Canal in course of construction, the contemplated Isthmus of Corinth Canal—all for saving circuitous journeys in passing from one sea to another—or, in the case nearer home, of the Manchester Ship Canal, for taking ocean steamers many miles inland.

But the old fight between the canal engineer and the railway engineer, or,

* "Minutes of Proceedings," Inst. C. E., vol. xlv., p. 107.

more properly speaking, between the engineer when he has his canal "stop" on, and the same individual when he has his railway "stop"—(you will see that I am borrowing a figure, either from *Dombey & Son*, where Mr. Feeder, B. A., is shown to us with his Herodotus "stop" on, or, as is more likely, I am thinking of the organs to be exhibited in the Second Division, "Music," of that exhibition of which I have the honor to be chairman)—I am afraid this is a long parenthesis, breaking the continuity of my observations, which related to the old rivalry between canal and railway engineering—I was about to say that this rivalry was revived, even in the case of the transporting of ocean vessels from sea to sea, for we know that our distinguished member, Mr. Eads, is proposing to connect the Atlantic and the Pacific Oceans by means of a ship railway across the Isthmus of Panama. He suggests that the largest vessels should be raised out of the water, in the manner commonly employed in floating docks, and should then be transferred to a truck-like cradle on wheels, fitted with hydraulic-bearing blocks (this being, however, not a new proposition as applied to graving docks), so as to obtain practical equality of support for the ship, notwithstanding slight irregularities in the roadway, while he proposes to deal with the question of changes of direction by the avoidance of curves and by the substitution of angles, having at the point of junction of the two sides, turntables on which the cradle and ship will be drawn; these can be moved with perfect ease, notwithstanding the heavy load, because the turntable will be floating in water carried in circular tanks. The question of preserving the level of the turntable, whether unloaded, partially loaded or loaded, is happily met by an arrangement of water ballast and of pumping. I cannot pass away from the mention of Mr. Eads' work without just reminding you of the successful manner in which he has dealt with the mouth of the Mississippi, by which he has caused that river to scour and maintain a channel 30 feet deep at low water, instead of that of 8 feet deep, which prevailed there before his skillful treatment. Neither can I refrain from mentioning the successful labors of our friend Sir Charles Hartley, in improving the navigation of that great European river, the Danube. I am sure we are all rejoiced to see that one of the lectures of the forthcoming series, that on "Inland Navigation," is to be delivered by him, and I do earnestly trust he will remember it is his duty to the institution not to leave important and successful works unreferred to because those works happen to be his own. I regret that time does not admit of my noticing the many improved machines for excavating, to be used either below water or on dry land. I also regret, for similar reasons, I must omit all mention of ship construction, whether for the purposes of commerce or of war, a subject that would naturally follow that of rivers and of ship railways and canals, and would have enabled me to speak of the great debt this branch of civil engineering owes to the labors of our late member, William Froude, and would have enabled me also to deal with the question of material for ships, and with the question of armor-plating, in which, and in the construction of ordnance, our past-president, Mr. Barlow and myself, as the two lay members of the ordnance committee, are so specially interested.

The mention of the armor-plates inevitably brings to our minds the consideration of ordnance, but I do not intend to say even a few words on this head of invention and improvement—a topic to which a whole evening might well be devoted—because, only three years ago, my talented predecessor in this chair, Sir William Armstrong, made it the subject of his inaugural address, and dealt with it in so masterly and exhaustive a style as to render it absolutely impossible for me to usefully add anything to his remarks. I cannot, however, leave this branch of the subject without mentioning, not a piece of ordnance, but a small arm, invented since the date of Sir William's address. I mean the Maxim machine gun. This is not only one of the latest, but is certainly one of the most ingenious pieces of mechanism that has been devised. The single-barrel fires the Martini-Henry ammunition; the cartridges are placed in loops upon a belt, and when this belt is introduced to the gun, and some five or six cartridges have been drawn in by as many reciprocations of a handle, the gun is ready to com-

mence firing. After the first shot, which must be fired by the pulling of a trigger in the ordinary way, the gun will automatically continue to send out shot after shot, until the whole of the cartridges on the belt are exhausted; and if care is taken before this happens to link on to the tail of the first belt the head of a second one, and another belt to this, and so on, the firing will be automatically continuous, and at a rate anywhere between one shot per minute and six hundred shots per minute, dependent on the will of the person in charge of the gun, the whole of the operations of loading, firing and ejecting the cartridge being performed by the energy of the recoil. This perfectly automatic action enables the man who works the gun to devote his whole attention to directing it, and as it is carried on a pivot, and can be elevated and depressed, he can, whilst the gun is firing, aim the bullets to any point he may choose.

Since 1862 the power of defending seaports has been added to by the application of submarine mines, arranged to be fired by impact alone, or to be fired on impact when (under electrical control) the firing arrangement is set for the purpose, or to be fired electrically from the shore by two persons stationed on cross-bearings, both of whom must concur in the act of explosion. These mines are charged with gun-cotton, the development of which owes so much to Sir Frederick Abel, while for purposes of attack the same material, not yet in practical use for shells, is taken as the charge for torpedoes, which are either affixed to a spar or are carried in the head of a submerged cigar-shaped body. By a compressed air or by a direct steam impulse arrangement these weapons are started on their course and are directed, and then the running is taken up by their own engines operating on screw propellers, driven by a magazine of compressed air contained in the body of the torpedo. Means are also provided to maintain the designed level below the water surface. The torpedo may either be projected from the war ship itself, or from one of those launches which owe their origin to our member, Mr. John Isaac Thorneycroft, who first demonstrated the feasibility of that which was previously considered to be impossible, viz., the obtaining a speed of twenty miles and over from a vessel not more than 80 feet long. Experiments have been carried on in the United States, by Captain Ericsson, to dispense with the internal machinery of the torpedo, and to rely for its traverse through the water upon the original impulse given to it by a breech-loading gun, carried at the requisite depth below the water level in a torpedo boat. This gun, having a feeble charge of powder at a low gravimetric density, fires the torpedo, and, it is said, succeeds in sending it many yards, and with a sufficient terminal velocity to explode the charge by impact. Also, in the United States, experiments have been made with a compressed air gun of 40 feet in length, and 4 inches in diameter (probably by this time replaced by a gun of 8 inches in diameter), to propel a dart through the air, in the front of which dart there is a metallic chamber containing dynamite. Although, no doubt, the best engineer is the man who does good work with bad materials, yet I presume we should not recommend any member of our profession to select unsuitable materials with the object of showing how skillfully he can employ them. On the contrary, an engineer shows his ability by the choice of those materials which are the very best for his purpose, having regard, however, to the relative facilities of carriage, to the power of supply in sufficiently large quantities, to the ease with which they can be worked up or built in, and to the cost.

Probably few materials have been found more generally useful to the civil engineer, in works which are not of metal, than has been Portland cement. It should be noticed that during the last twenty two years great improvements have been made in the grinding and in the quality of the cement. These have been largely due to the labors in England of our member Mr. John Grant, to the labors of foreign engineers following in his footsteps, and to the zeal and intelligence with which the manufacturers have followed up the question, from a scientific as well as from a practical point of view, not resting until they were able with certainty to produce a cement such as the engineer needed. I do not know that there is very much to be said in the way of progress (so far as the finished results are concerned) in the materials

which Portland cement and other mortars are intended to unite. Clean gravel and ballast and clean sand are, I presume, very much the same in the year 1884 as they were not only in the year 1862, but as they were in the year 1. The same remark applies to stone and to all other natural building materials; and, indeed, even the artificial material, brick, cannot in these days be said to surpass in quality the bricks used by the Romans in this island nineteen hundred years ago, but as regards the mode of manufacture and the materials employed there is progress to be noted. The brick-making machine and the Hoffmann kiln have economized labor and fuel, while attempts have been made, which I trust may prove successful, for utilizing the clay, which is to be found in the form of slate in those enormous mounds of waste which disfigure the landscape in the neighborhood of slate quarries. Certain artificial stones, moreover, appear at last to be made with a uniformity and a power of endurance, and in respect of these qualities, compare favorably with the best natural stone, and still more favorably having regard to the fact that they can be made of the desired dimensions and shape, thus being ready for use without labor of preparation.

Reverting to natural materials, there remains to be mentioned that great class, timber. In new countries the engineer is commonly glad to avail himself of this material to an extent which among us is unknown. For here, day by day, owing to the ready adaptability of metals to the uses of the engineer, the employment of wood is decreasing. Far, indeed, are we from the practice of not more than a hundred years ago, when it was not thought improper to make the shell of a steam-engine boiler of wooden staves. The engineer of to-day, in a country like England, refrains from using wood. He cannot cast it into form, he cannot weld it. Glue (even if marine) would hardly be looked upon as an efficient substitute for a sound weld; and the fact is that it is practically impossible to lay hold of timber when employed for tensile purposes so as to obtain anything approaching to the full tensile strength. If it be desired to utilize metals for such a purpose, they can be swollen out into appropriate "eyes" to receive the needed connection; but this cannot be done with wood, for the only way of making an enlarged eye in wood is by taking a piece that is big enough to form the eye, and then cutting away the superfluous portion of the body. Moreover, when too much exposed to the weather, and when too much covered up, wood has an evil habit of rotting, compared with the rapidity of which mode of decay the oxidizing of metals is unimportant. Further, one's daily experience of the way in which a housemaid prepares a fire for lighting is suggestive of the undesirability of the introduction of resinous sticks of timber, even although they may be large sticks, into our buildings. Many attempts, as we know, have been made to render timber proof against these two great defects of rapid decay and of ready combustibility, and as it appears to me, it is in these directions alone one can look for progress in connection with timber. With respect to the first, it was only at the last meeting of the institution we presented a Telford medal and a Telford premium to Mr. S. B. Boulton, for his paper "On the Antiseptic Treatment of Timber," to which I desire to refer all those who seek information on this point. With respect to the preservation from fire of inflammable building materials, the processes, more or less successful, that have been tried are so numerous that I cannot even pretend to enumerate them. I will, however, just mention one, the asbestos paint, because it is used to coat the wooden structures of the inventions exhibition. To the employment of this, I think, it is not too much to say those buildings owed their escape, in last year's very dry summer, from being consumed by a fire that broke out in an exhibitor's stand, destroying every object on that stand, but happily not setting the painted woodwork on fire, although it was charred below the surface. I do not pretend to say that a surface application can enable wood to resist the effects of a continued exposure to fire, but it does appear that it can prevent its ready ignition.

Leaving the old world materials of stone and wood, let us come, not only to the bronze age, but to the iron age, and direct our attention during a few minutes to the improvements which, in twenty-two years, have been made—and

first to deal with that form of iron known as steel. I am aware that I am laying myself open to a charge of having committed a most tremendous "bull," but I am prepared to defend my form of speech, on the very strong ground that no one can say, speaking as a metallurgical chemist, where the dividing line is between commercial iron and commercial steel, for it is quite certain there is material which would be currently bought and sold and used as steel, which is more near to pure iron than is other material which would be commercially bought and sold and used as iron. It is now nearly eight years since I delivered, at the Royal Institution, a lecture on "The Future of Steel," and every year that has passed has justified the opinions I then ventured to put forward as to the way in which steel made by fusion would supersede iron made by the puddling process; and I am not afraid to repeat my prophecy that the time will come when the use of iron made by that process will be restricted to the manufacture of the small articles produced by the hand labor of the village blacksmith, for whose art its plastic character and ready power of welding eminently fit it. Probably the first great revelation in steel manufacture was the exhibition of the ingots, with other products shown by Krupp in the '51 exhibition; it soon became known, however, that these exhibits, after all, gave us no further information than this, viz., that it did not follow because the limit of the charge of a crucible might be 50 or 60 lbs., the limit of the size of the ingot must also be 50 or 60 lbs.; in fact, the world was shown that more than one pot of steel might be discharged into the same ingot mould; indeed, that hundreds of pots might be. Do not imagine for one moment I am depreciating this step. It was an enormous one at the time when the production of fused steel involved the employment of the crucible. But, according to my judgment, the making of steel in crucibles is not so satisfactory a mode of obtaining uniformity in large masses, as is either of the other two great systems of manufacture, I mean the Bessemer and the Siemens, the two processes which have changed the whole complexion of the iron industry. For years after 1862 we had papers at this institution upon the question of steel rails, and we had it solemnly stated that the suggested economy in using these was an apparent economy only, for when interest was taken into account, having regard to the extra cost of steel over iron, which must always prevail, it would never pay to employ steel rails, and the true function of steel in the permanent way would be to restrict its use to points and crossings. Now, it would be difficult to induce anyone to believe that an engineer was serious if he specified for wrought-iron rails, as it would be known that he would have to pay more money to obtain this inferior material. Important as the subject is, time compels me to refrain from further allusion to it, and forces me to conclude this head of my address with the physician's (Abernethy) well-known advice to his patients, "Read my book," *i. e.*, my lecture at the Royal Institution, to which, however, I must add one word, and that is, I must here refer to the important improvement made since the date of that address by the process of Messrs. Thomas and Gilchrist, by which it has been rendered possible to employ successfully, in the production of steel, iron derived from ores which, prior to the date of this invention, had been found wholly inapplicable for the purpose.

In the manufacture of pig iron, improvement has been effected by increasing the dimensions of the furnaces, and (thanks to Mr. Cowper in the outset, followed by others) by increasing the temperature of the blast, and by the closer application of chemistry to the industry, by the total closing of the bottom of the furnace, and by the increased use of the waste gases. From these improvements an economy and a certainty of production have resulted, leaving little to be desired; while it is to be hoped that another waste product—that of blast furnace slag—will be converted to various useful purposes.

I have varied the usual order by taking the iron age before the bronze. To revert to the bronze—the mysterious influences that a very small percentage of some material will exercise upon the quality of the great bulk of another material, with which it may be united, are well shown in the case we have been considering—that of steel—where a few tenths of one per cent. of carbon added to the iron suffices to change the iron

into steel. We are not surprised, therefore, when we find that other metals may have their qualities improved, for many useful purposes, by judicious alloy; and in this way the metal, copper, so long used in its alloyed condition of "gun-metal," has within the last few years been still further improved by alloying it with other substances, and thus making it into the now well-known articles "phosphor bronze" and "manganese bronze"—very useful materials to those of our members engaged in the construction of machinery. So closely allied to the consideration of the nature of a material is that of the means of producing it in the desired form, that one naturally passes thereto. As long as small masses had to be dealt with, and as long as those masses were of a plastic character, it was possible to successfully employ the hand-hammer, the sledge-hammer, and, later on, the steam-hammer; but with the increased dimensions of the main shafts of engines, and of the solid forgings for the tubes of cannon, obtaining at the present day, and having regard to the fact that these are composed of steel, the operations of light steam-hammers are absolutely harmful, tending to produce internal flaws, and the blows of even the heaviest class of hammers are not so efficacious as is pressure applied without blow. I think the time is not far distant when (following the lead of Sir Jos. Whitworth) all steel in its molten state will be subjected to pressure, not with the object of making the metal more dense, but with the object of diminishing the size of any cavities containing imprisoned gases; if this is not done, then some other mechanical means will be employed to get rid of the cavities altogether, and thus to produce (without variations in the constituents of the steel) a casting that shall be practically if not absolutely free from blowholes, and so that such casting, when afterwards forged by pressure and not by percussion, may be thoroughly trusted to contain no latent defect. One of the difficulties that was foreseen in the outset of the employment of steel for tires, was the difficulty of welding the ends of a steel tire bar, after it had been bent into the hoop form. I was, I believe, one of the very earliest to suggest the making of tires in the hoop form, and so not only to avoid the cost and risk of welding, but also to avoid the waste on each tire bar, arising from what was known as the "crop end." I read a paper on this subject before section G of the British Association, at the Birmingham meeting in 1865, and I then prophesied that, in a very few years from that time, a welded tire would be unknown—a prophesy which has been amply fulfilled; but I also pointed out that, so far from its being the right way to set about the manufacture of a hoop by beginning with a straight bar, then bending it, and then welding it, the manufacture in the hoop form would be the proper one to adopt, even if the object were eventually to produce a straight bar, such as a rail; for if this were done the relling would be continuous, and there would be no "crop end," no waste therefrom, and no fear that in order to render the waste as little as possible, there would be retained at the ends of the rail—its most vulnerable parts—metal of an inferior character. In this same paper I showed that the right way of making boiler-shells and boiler-flues would be by the hoop system and by endless rolling, thereby avoiding the longitudinal seams, which, after the very best has been done that can be done, reduce the effective strength of the boiler plate by one-fourth or one-third, and commonly reduce it by one-half. I will refer my hearers to Vol. XX. of the *Engineer*, p. 200, where the paper and the accompanying diagrams are given.

The subject of steam boilers brings one naturally to the consideration of that which still remains the great source of motive power—the steam engine. Here since 1862 it is difficult to point out to any great substantive novelty, but these machines have been more and more scientifically investigated, and the results of such investigation have been practically applied, and have been attended with the anticipated advantages. The increase in initial pressure, the greater range of expansion, the steam-jacketing of the vessels in which the expansion takes place, have all led to economy, so that double-cylinder, non-condensing engines are now currently produced, which work with a consumption of only 2¼ lbs. of coal per gross indicated horse-power, or 2.7 lbs. per horse-power delivered off the crank shaft, equal to eighty-three millions of

duty on the Cornish engine mode of computation; and when these high results are augmented by the employment of surface condensation, an indicated horse-power has been obtained for as little as 1½ lb. of coal, and it is commonly obtained, in daily work, for from 2 lbs. to 2½ lbs. But the engineer using steam as his vehicle in a heat motor still has to submit to the chagrin of seeing the largest portion of the heat pass away unutilized. This defect has for years attracted the attention of scientific engineers. Indeed, we know that more than thirty years ago our lamented friend, William Siemens, devoted his great powers to the production of a regenerative steam engine by which he hoped to decrease this loss, but at that time he was not successful in producing a practical machine. The labors of those who, following Stirling, have endeavored, by employing air as the vehicle of heat, to obtain better results, have succeeded in producing very economic machines, and machines of practical utility, but hitherto only applicable where small power is required. There is, however, another form of heat motor which, while vainly essayed during fifty years, has within the last eight years come into common use, and the application of which in cases requiring anything up to 30 indicated horse-power is daily increasing. I need hardly say that I allude to the gas engine. By a happy change in the mode of burning the mixture, and of utilizing the heat thereby generated, the injurious shock of the early forms of gas engine, and the large consumption of gas which caused these earlier forms of engine to be discarded after trial, were obviated, a notable instance being in the engine propelling the fan that ventilates this room, which, after a short time, was pulled out and replaced by an hydraulic engine. According to the *Mechanics' Magazine* of August 10, 1866, page 87, the French engineer who tried a Hugon gas engine, found that 74 cubic feet of gas per indicated horse-power per hour were required; this is now replaced by the 20 to 23 feet per indicated horse-power consumed in the engines of the present day. With the low price of gas commonly prevalent in England, this consumption does not cost more than some seven-eighths of a penny per horse per hour. I am aware it may be said that with coal, even at the London prices of £1 per ton, I might use a steam engine having the low economy of 8½ lbs. of coal per indicated horse-power per hour before I should be called on to spend seven-eighths of a penny per indicated horse-power per hour for fuel; you would be astonished to hear, however, that in an investigation instituted last year by the corporation of Birmingham, when considering whether they should approve of a proposal to lay down power-distributing mains throughout their streets, it was found on indicating some six non-condensing steam engines taken indiscriminately from among users of power, and ranging from five nominal horse-power up to thirty nominal horse-power, that the consumption in one instance was as high as 27.5 lbs., while it never fell below 9.6 lbs., and the average of the whole was as much as 18.1 lbs. This heavy consumption largely arose from a prevalent defect, one I have frequently pointed out, that of too great cylinder capacity; for unless a non-condensing engine is admirably designed, and made with the object of using very high expansion, there is nothing so wasteful as the employment of that which the buyer of an engine looks upon as an advantage—very great cylinder capacity. The result of such a construction being that the initial pressure of the steam, in the cylinder of the ordinary small-power non-condensing engine, is not more than 20 to 30 lbs. above atmosphere, a condition of things wholly incompatible with economy. But even assuming that the user of a gas engine were entitled to compare it with a non-condensing steam engine consuming only some 5 lbs. of coal per indicated horse-power per hour, and demanding, therefore, at 1s. per cwt., only one half-penny for the purchase of coal, this difference in cost is well repaid by the saving of boiler space, of the wear and tear, and of the renewal of the boiler, of the consumption of coal while getting up steam, and during meal times, and the saving in the engineer's or stoker's wages; and on public grounds there are the advantages of freedom from boiler explosion, and of cessation of smoke production.

I have spoken of gas engines hitherto, as though, like hot-air engines, they were

necessarily restricted in their dimensions, but this is not so; engines are now being made to develop 50 horse-power; and further, be it remembered, that when used on a large scale, so that it would pay to have an attendant devoting his whole time, there is no need to work them with illuminating gas from the street mains, they can be driven by producer-made gas on Dowson's system, and when worked in this way a pound and a-half of "culm" will give one horse-power, and one lad is sufficient to manage a gas-producing apparatus of a size adequate to provide for engines developing 300 indicated horse-power. I ventured to say, at the meeting of the British Association at York, in 1881, when giving a partial review of that which had happened in engineering in the fifty years from the foundation of the association, that unless some wholly unexpected improvement were made in the steam engine, those who lived to see the celebration of the centenary of the association in 1931, would find the steam engine had become a curiosity, and was relegated to museums, for I could not believe steam would continue to be the vehicle for transmitting heat into work.

A motor has been recently tried where no fuel is employed directly, but where a boiler, being filled with water and steam under pressure, has its heat maintained by exposing caustic soda, contained in a vessel surrounding the boiler, to the action of the waste steam from the engine, the result being that, as the moisture combines with the caustic soda, a sufficient heat is developed to generate steam and keep the engine working for some time. The trials have been made with the motor for propelling a launch, and I believe with one for working a tram-car. It may be we have here a source of power in a portable form, useful for the purposes I have mentioned, and for others analogous thereto. It hardly needs to be said fuel has eventually to be employed to drive off the moisture from the soda, and thus to bring it back to its caustic condition.

I cannot pass away from this brief allusion to heat motors without expressing my gratitude to those lecturers who addressed us on "The Mechanical Applications of Heat" last session, and especially to our member, Mr. William Anderson, for his lecture in that course on "The Generation of Steam and the Thermo-dynamic Principles involved." Let me tell those of you who do not already know it that Mr. Anderson has still three lectures to deliver, out of a course of six lectures which he is giving at the Society of Arts, on "The Conversion of Heat into Useful Work," and permit me to advise all those members of this institution who can possibly do so to attend (as I hope to be able to do) the remainder of those most clear and instructive lectures.

There is one indisputable heat motor I have omitted, viz., that wherein power is obtained directly from the sun's rays. Attempts have been made during the last twenty-two years in this direction, but we enjoy so little powerful sunshine in England, and the question is still in such a thoroughly experimental stage, that I think I must not take up your time by any consideration of it. With respect to other motors, viz., those driven by wind or by water, not commonly looked upon as heat motors (although, in truth, there would be no such agencies without heat), but on these there is not time to say much, I will merely call your attention to the improvement in water-wheels in France, an improvement by which it is asserted that as much as 85 per cent. of all the energy residing in a low fall of water has been converted into power; a result due to the decreasing of the speed of the periphery of the wheel, and to the making of the buckets very narrow and of great depth. In turbines, also, there has been considerable development in these twenty-two years, and they now take their place as very efficient motors, possessing many advantages, where, on the one hand, a very high fall of water has to be utilized, or where, in the case of a low fall, great difference in the working head, and in the level of the tail water, have to be provided for.

With respect to the power of the tide, I, for one, have been very much fascinated with the scope there appeared to be for engineering in utilizing tidal power, especially where there was a great ebb and flow, and I have on former occasions expressed some sanguine opinions as to the practical use that could be made of this source of power; but, being called upon to look into the question, I found that

as in these days of competition, very few businesses needing motive power can allow their plant to remain idle for nearly half the working day, and that as there is an objection to remedying this condition of things by working when possible, both during the night tide and during the day tide, this was an obstacle in most cases to the use of tidal power. Further, when it was sought to preserve continuity of action by providing a series of reservoirs, the outlay needed was so large that the mere interest on it would pay for the fuel for the steam engine. I am afraid, therefore, that, except for certain cases—such as pumping of water into a reservoir, or charging of so-called storage batteries, or matters of that kind not connected with ordinary manufacture—this source of power is not likely to compete commercially with heat motors until coal is very much dearer. The periodic intermittency being a sufficient bar to the employment of tidal motors, it is not to be wondered at that the (proverbial) uncertainty of the wind causes motors which have to be driven by it to be disregarded as substitutes for the steam engine. I have, however, said elsewhere I think it is well worth considering whether wind motors could not be employed as adjuncts to steam engines, diminishing the load upon them or laying them idle altogether, according to whether there was a light or a strong breeze blowing.

The uncertainty as regards the obtaining a sufficient breeze, which prevents the wind from being a trustworthy source of power, aggravated by the further uncertainty as to the direction in which the breeze would blow when it did come, has rendered the air, as a medium for navigation, even more untrustworthy. During the last few years, however, a new locomotive agent has been prominently brought forward—I mean a balloon capable of being propelled and steered, or, as it has been termed, a "dirigible" balloon. Many persons have fancied that it is impossible to propel a balloon through the air, but this, as I need hardly tell those who understand mechanics, is entirely a fallacy. The reasons why the early attempts to steer balloons failed, were practical and not theoretical, and they have been removed by recent mechanical improvements. The first really successful effort was made by M. Henri Giffard, the ingenious inventor of the injector, all reference to which I have omitted, however, because of its being slightly anterior to 1862. In 1852 this gentleman ascended in an elongated balloon propelled by a steam engine working a screw propeller; and he was followed twenty years later by M. Dupuy de Lome (the Government naval architect of France), who, however, used hand power. The speed through the air in these trials was about six miles an hour, and the steering power was fully obtained. Taking these and other experiments as data, my friend and fellow member, Dr. Pole, to whom I am indebted for the information on this subject, published in our "Proceedings" in 1882 a full investigation of the problem, which led him to believe that a velocity of 25 to 30 miles an hour might be attained; and since that time further trials have been made in France by Messrs. Tissandier, Renard, and Krebs, who, using electric power, have already accomplished half the predicted speed, with a promise of much further development, when more experience has been gained with the practical details. I fear that the rapid and changeable motion of the medium in which balloons have to move will prevent this mode of locomotion from ever having a wide application, but there may undoubtedly be particular circumstances in which it would be useful, such, for example, as the exploration of new countries, or as the present Egyptian campaign. I strongly suspect that if our lively neighbors, instead of ourselves, had been invading the Soudan, they would long before this have had a "dirigible" balloon looking down into Khartoum. Let me refer all those who take an interest in this question to an earlier article by Dr. Pole, which was published in the *Quarterly Review* of July, 1875. This article is still considered a "classic" on the subject.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The following is the list of officers for 1885:

President: FREDERICK GRAFF.

Vice-Presidents: GEORGE S. GREENE, JR.; THOMAS J. WHITMAN.

Secretary and Librarian: JOHN BOGART.

Treasurer: J. JAMES R. CROES.

Directors: THEODORE COOPER, WILLIAM R.

HUTTON, WALTER KATTE, O. CHANUTE, F. W. VAUGHAN.

FEBRUARY 4TH. 1885.—President Frederick Graff in the chair. The death, on February 3d, of Theophilus Sickels, M. Am. Soc. C. E., was announced, and the appointment of a committee to prepare a memoir was authorized.

The paper by E. Sweet, M. Am. Soc. C. E., State Engineer of New York, on the Radical Enlargement of the Artificial Waterway between the Lakes and the Hudson River, was discussed.

Mr. Sweet's paper refers to the fact that for twenty years after the enlargement of the Erie Canal to its present capacity it held a prominent place in our internal commerce. Contemporaneous with its enlargement began the wonderful development of the railway system which has since absorbed much of our best engineering, executive and administrative ability, and much of the available capital of our country, to the exclusion of water routes. The Erie Canal, unchanged for thirty years, except in its gradual deterioration, with unimproved equipment and mode of operation, is gradually losing its capacity for usefulness and its influence upon the problem of transportation. Thirty years ago the Erie Canal carried nine-tenths of the freight between Buffalo and New York; now it carries less than one-fifth of it. Minor improvements have been urged, but its inadequacy to meet the present and prospective wants of northwestern commerce has become so manifest as to demand a far more radical improvement.

To become the permanent highway for this commerce, the canal must have capacity to carry from Lake Erie to the deep waters of the Hudson the largest vessels now navigating the lakes, or as those vessels may probably be enlarged in the future. The canal should be at least 18 feet deep, 100 feet wide at bottom, and with locks 450 feet long and 60 feet wide. It must receive its water from the lake and discharge it into the Hudson. While the changes required are radical, they are entirely practicable and involve no very serious difficulties. The essential change of profile would be in extending the Rome level westward to Lock 57, near Newark, in Wayne County. The only difficulty of importance would be at and near the crossing of the Seneca River, and this would be reduced by a change of location at that point. Probably the best results would be obtained by an entirely new route from Syracuse eastward. From the vicinity of Utica, the Mohawk river should be canalized to the Hudson.

The plan may be summarized as the widening, deepening and necessary rectification of the worst curvatures of the present canal from Buffalo to Newark, about 130 miles: the construction of a new canal from Newark to Utica, about 115 miles; the canalization of the Mohawk river, from Utica to Troy, about 100 miles, and the improvement of the Hudson river from Troy to Four Mile Point, in Coxsackie, a distance of about 30 miles.

The elevation of the western level of the canal, being governed by the surface of Lake Erie, must secure the required depth wholly by deepening, while the profiles of the levels from Lockport east can be adjusted to meet the economical requirements that will be disclosed by detailed surveys.

The first level, from Buffalo to Lockport, will be 32 miles long. Descending from this level at Lockport, by two locks, each of about 25 feet lift, the second level of the canal will be reached. This level, 64 miles in length, will extend to Brighton, where, descending by two locks of about 24 feet lift, we reach the third level of the canal, extending from Brighton to Macedon, 20 miles; there, descending by a lock of about 20 feet lift, we reach the fourth level, extending from Macedon to Newark, 12 miles, where, by a lock of about 20 feet lift, is reached the level of the proposed new canal, to extend from Newark to Utica, about 115 miles, which will be the fifth and longest level of the new canal. From that point the Mohawk river (except at Little Falls and Cohoes, where combined locks will be required) can best be canalized through locks of 10 or 12 feet lift, making pools having an average length of about 5 miles each.

The construction of this great artificial river, more than 300 miles long, is a vast enterprise. Its cost may be roughly assumed at $125,000,000 to $150,000,000, and its probable tonnage at 20,000,000 to 25,000,000 of tons per annum.

The first requisite to the possible inauguration of this enterprise is a careful system of surveys. The canal should be under the control of the State of New York, and that State should without delay cause the necessary surveys and estimates to be made.

By means of the enlarged canal, cargoes would be carried from Chicago to New York in less time and at less cost than can now be done by canal from Buffalo to New York. With our widespread territory, cheap transportation is a necessity; and the Erie Canal should be given the necessary capacity to effectually secure that result.

A discussion followed which was to be continued on February 18th.

ENGINEERS' CLUB OF PHILADELPHIA.—RECORD OF SEVENTH ANNUAL MEETING, JANUARY 10TH, 1885.

President Ludlow delivered the Annual Address.

He drew a rapid picture of the universality of modern activity and means of communication, and briefly depicted the astonishment of an ancient philosopher, could he be recalled to participation in the life of to-day. The restless activity of mankind in every corner of the globe is directed by careful study and observance of natural laws. The specialization of knowledge naturally follows from its vast accumulations, but the engineer should be careful to thoroughly ground himself in fundamental principles, and maintain his acquaintance with kindred lines of thought. The progress of civilization has been effected by the discovery and utilization of natural laws, by means of which man has emerged from a

savage state and become the ruler of the earth, pressing into service all the powers of nature and using them to his advantage.

The engineering history of the past year was briefly gleaned over and the principal features mentioned, including the improvement of the Mississippi River and the construction of tidal harbors. The prospect of a revival of military engineering in America was touched upon, with the accompanying development of the metallurgical arts in the construction of guns and armor.

Under the general title of Sanitary Engineering in the broadest sense are included nearly all those branches which affect the existence and prosperity of cities; water supply, sewerage, bridges, street cleaning and paving, etc.

Speaking of Philadelphia engineering, he said: "While serious attention has been drawn to the improvement of its water supply, the prospect of actually entering upon the construction of the necessary works is vague and distant. The streets are covered with a pavement which, in all well-regulated cities, has long since been abandoned as costly to traffic, impossible to clean, permeable to street fluids, noisy and hopelessly disreputable. But one sound bridge across the Schuylkill exists. The Fire Department labors under the most serious disadvantages from lack of water. The city sewers are without means of flushing or cleansing, and many are so constructed as to waste their contents. In consequence the foul matters are either to a great extent stored up under the pavements to give out poisonous vapors, or saturate the soil in which the dwellings are constructed. . . . I look forward with the eye of hope to the future of the city; with smooth, noiseless, water-tight pavements, ample facilities for connecting the population of West Philadelphia with the old city; rapid transit of the population from point to point in decency and comfort, clean street., healthful homes, pure air and pure water I anticipate the reconstruction of the sewers upon a well considered and scientific system."

A project for the useful disposal of Philadelphia sewage upon the sandy and thirsty farms of New Jersey by means of intercepting sewers, a conduit beneath the Delaware and a distributing aqueduct eastward, was briefly sketched. The several scientific assemblies in Philadelphia during 1884 are mentioned, and in conclusion the Club is congratulated upon its great prosperity during the year, and a continuance for the future anticipated.

President, J. J. de Kinder; Vice-President, Joseph N. DuBarry; Secretary and Treasurer, Howard Murphy; Directors, T. M. Cleemann, Frederic Graff, Rudolph Hering, William Ludlow and Henry G. Morris.

ENGINEERING NOTES.

ON THE RELATIVE COST OF RETAINING-WALLS BUILT IN BRICK AND IN COLUMNAR BASALT —By P. H. KEMPER.—Mr. Kemper collected data from different public works, executed in brick and in basalt, of late years, and compared their actual cost prices. Basalt being the heavier material, a saving can be effected in the thickness of walls by employing this material. On the other hand, the adhesive strength of mortar on basalt is only about half of that on brick; but the superior weight restores in friction on the joints what may be lost in adhesion.

The author instances works executed, amongst others, the basalt wall of the lock at Flushing, where 27 per cent. was saved, and the quay-wall at Maasluis, where 11 per cent. was saved by building in basalt instead of in brick. For these works the actual cost of 1 cubic meter of basalt masonry was 16.40 florins; 1 meter cube of klinker brickwork, 18.88 florins; of stock-bricks, 17.67 florins. At the lock at Vreeswijek the price paid for basalt masonry was 15.90 florins, for brickwork 16.07 florins per cubic meter. The mortar used for basalt is sand concrete, at 12 florins per cubic meter; for brickwork, mortar of equal parts of shell-lime and Rhenish trass.

In walls up to 4 meters high the saving is not appreciable, but it rapidly increases with the height.

According to experience, columnar basalt is preferable to brick:—in walls exposed to damp, such as retaining-walls for quays and locks, where the water line is oscillating and the backing wet, also because basalt is of greater specific gravity; it is not liable to crumble away in frosty weather; its cost of maintenance is considerably less; and, in cases of settling and subsequent repairs, the material is not lost by the breaking out, but can be used again, which is not the case with brickwork. For large works and walls of great height basalt gives a better appearance than brick.

PROPOSED RAILWAY BRIDGE ACROSS THE STRAITS OF MESSINA.—This design is put forward by the directors of the Novara-Pino and the Genoa-Acqui-Asti Railways. The bridge is to have three steel arches of 3,280 feet from center to center of piers and two half arches of 1,640 feet. The depth of the straits where the bridge crosses is 360 feet. The piers are to be built of granite in cement, founded on masses of granite thrown down into the sea, and brought up to within 65 feet of the surface. The piers are to be 33 feet high from water to springing level, and above this they are to be built of immense blocks of granite to a further height of 62 feet for the arches to abut against. The width of the piers is to be 236 feet, the clear span of the arches 3,083 feet, and their versed sine 328 feet. The thickness of the arch at the springing is 65 feet, which is reduced towards the crown. The road is carried on a longitudinal girder 10 feet in depth, connected with the arch by lattice bracing.

The width of the bridge is 65 feet at the center, widened out to 197 feet at the springing, to give lateral resistance to the structure and to enable it to withstand wind-pressure. The arch is to be erected without scaffolding or centers. Above each pier a temporary structure is to be erected similar to the lattice work between the horizontal girder and the arch but in an inverted position, and forming with the latter a pair of cantilevers. The operation of erecting the arch is

to be carried out thus: First, the arrangement above described is to be put up, consisting of the permanent lattice-work and the temporary inverted lattice, forming a cantilever on each side of the pier 690 feet in length. Next a length of 525 feet of the main arch is to be erected on each side of the pier. The erection is then to be extended a further length of 260 feet on each side. An additional length of 130 feet is then added, being as before supported by the longitudinal girder and lattice-work, which are kept in advance of the arch. Similar processes are repeated till a length of 1,230 feet on each side of the pier is in position, leaving a length of 410 feet on each side to complete the two half arches, or 820 feet in each arch; but this, being the lightest part of the structure, can be put together on the part already erected, and then rolled out and lowered into position.—*Abstracts of Inst. of Civil Engineers.*

IRON AND STEEL NOTES.

Separation of the Phosphates from Slag after the Thomas-Gilchrist Process.—The increasing adoption of the Thomas-Gilchrist process in Germany during the past few years had turned the attention of chemists in that country to the nature of the slag or refuse products of this new development of the iron manufacture. It was ascertained that amongst other things fully 20,000 tons of phosphoric acid were every year being thrown away with this refuse because no method had been discovered of separating it at a profit. Numerous experiments have been made during the last five years with a view to hitting upon a sufficiently cheap process, but hitherto these attempts were all unsuccessful. A few months ago, however, Professor Scheibler, of Berlin, succeeded in solving the problem. An analysis of the slag from the Thomas-Gilchrist process at one of the chief ironworks in Germany, showed its constitution to be as follows: Silicic acid, 6.23 per cent.; carbonic acid, 1.70 per cent.; sulphur, 0.56 per cent.; phosphoric acid, 19.33 per cent.; iron, 9.70 per cent.; manganese, 9.50 per cent; lime, 47.60 per cent.; and oxide alumina, 2.58 per cent. Other analyses did not materially differ from this; the quantity of phosphoric acid only varied between 15½ and 20 parts in the 100, while the silicic acid varied from 6 to 11 per cent., the proportion of lime being always nearly 50 per cent. According to the Scheibler process, only the earth phosphates and the silicates are brought into solution. The proportion of metallic oxides found in the solution is of no practical consequence, and thus the quantity of acid employed in the operation is reduced to a minimum. The phosphoric acid can be precipitated directly from the solution in the form of double basic phosphate of lime. It comes out in the shape of a powder in the finest state of division, and owing to the readiness with which in this form it is taken up by the roots of plants, these phosphates furnish at once a very valuable manure without any further treatment. On the other hand, the Scheibler process leaves the metallic substances and part of the earthy bases undissolved in the slag, and since the silicic acid is nearly all taken out with the phosphates, the refuse that remains after the operation furnishes a useful material for blast furnaces and other purposes.

The Krupp Works at Essen.—The great iron and cannon founding establishment of Herr Krupp at Essen is constantly enlarging its space and *personnel*. In 1860 it contained but 1,764 workmen, and this number had increased by 1870 to 7,084, while at the present time it is over 20,000; if also the women and children dependent on the establishment are included, a population of no less than 65,381 is gathered together, of which 29,000 persons are actually living in houses belonging to the works. The various departments of the Krupp undertaking are eight in number, and embrace the workshops at Essen, three collieries at Essen and Bochum, 547 iron mines in Germany, mines in the north of Spain, in the neighborhood of Bilbao; the smelting furnaces, a trial ground of 17 kilos. at Meppen for proving cannon, together with others at different places with an area of 7½ kilos. There are 11 smelting furnaces, 1,542 puddling and heating furnaces, 439 steam boilers, and 450 steam engines of 185,000 horse-power. At Essen alone the works connected with rolling stock comprise 59 kilos of rails 28 locomotives, 883 wagons, 69 horses, 191 trolleys, 65 kilos. of telegraph line, 35 telegraphic stations, and 55 Morse apparatus.

Removing Red-Shortness from Iron.—The iron ore of Cornwall, Pennsylvania, is rich in copper and sulphur. The greater portion of the latter is removed by roasting; but the copper remains, and, in smelting, alloys the iron with from 0.75 to 1.25 per cent., according to analyses made by a prominent steel-works chemist. It is very red-short or brittle at a red heat—a fact so well established that the trademark of the North Cornwall furnaces is C. R. S., standing for Cornwall red-short. This metal is used in the Bessemer process, mixed with good hematite pig-iron to some extent; but until recently it has never been used in the open-hearth process, as the product has been too red-short for hammering or rolling. The iron being in large and cheap supply, it became desirable to remove the red-short property in a simple and economical manner; and with that end in view, some was sent during the last summer to Bellefonte, Pennsylvania, to be experimented with in the open-hearth furnace there. The first trial, conducted in accordance with the plan of Mr. James Henderson, was made with one-half weight of Cornwall No. 3 pig-iron, half rail crop-ends, and 5 per cent. of iron ore, charged upon silica brick at the rate of 60 pounds of brick per ton of metal. The bricks were burnt and were composed of ninety-five parts of sand and five parts of lime by weight. The bricks were charged on the hearth, which was highly heated and ready for charging the pig-iron. The latter was placed on them, and the crop-ends on the iron. By the time the pig melted, the brick became partly fused and stuck fast to the

bottom and remained there, and gradually melted away during the decarbonization of the metal on the top of them, and passed up through the metal, removing the red-shortness. The second trial was without the silica brick, and the metal was too red-short to be of use. The trials with the silica brick were continued over several weeks, using several car-loads of this metal with uniformly satisfactory results, whether all pig and ore without scrap was used, or pig, ore and scrap. When sand alone was used, it removed the red-shortness, but a portion adhered to the hearth, and gradually raised it, so that it became necessary to cut it out afterward with lime. The better way is, therefore, to mix the lime with the sand at the start, and keep the hearth from rising by accumulation. A brick made of ninety-five parts of sand and five parts by weight of lime, mixed with water containing glucose, in the proportion of twenty parts to one by bulk and air-dried, is the most suitable for the use, as the brick insures the hearth being kept at its normal state. The ingots made from the metal rolled directly into shapes equally well, and tested as well as others that had been previously bloomed. The Allentown Rolling-Mill Company rolled some of these ingots, and reports the elastic limit 42,867 pounds and the tensile strength 66,289 pounds per square inch, with 25 per cent. elongation in 8 inches. This metal, when rolled into plates, punches cold without cracking, and doubles over upon itself at all temperatures without any crack at the bends, and for boiler plates is in every way equal to those made from the most costly materials. From this, it appears that, while silica in excess prevents removal of phosphorus, it acts in a contrary way when used with iron containing copper, and removes the copper.—*Engineering and Mining Journal.*

RAILWAY NOTES.

Railway Brakes in Germany.—Some time since we announced that the Baden State Railways had adopted the Westinghouse brake. It is true this step was not taken until they had experienced the terrible calamity at Hugstetten, where 72 people were killed and over 100 injured, though in this they were no worse than most people, who are proverbially desirous of shutting the stable door when the horse is gone. It would seem, however, that the Wurtemburg State has decided to follow the example of Baden, in adopting the Westinghouse brake, and without waiting for the occurrence of a disastrous accident. The work is to be carried on as rapidly as possible, and all the Continental railways will no doubt before many years be equipped with automatic air pressure brakes. It is one of the many curious features in the brake question that no railway company appears to consider that the experience of other companies should affect their own action. So long as they have themselves escaped without any disastrous consequences, they maintain the hope that the experience of others will never be their own, and this, whether lives have been lost by the want of efficient brakes, or, on the other hand, saved by the special qualities of a particular appliance. So far as this country is concerned, there is just as little provision upon many lines for meeting a calamity like that of Penistone, as there was previous to the disastrous day in July, when that fearful accident took place.

Fireless Tramway Engines.—The system of tramway haulage by fireless locomotives has been tried on a very considerable scale in Batavia, and has given so much satisfaction that it is contemplated to extend it. The Batavia Steam Tramway Company owns a line divided into two portions; the first, from Batavia to Kramat, having a length of 8 kilometers (5 miles) laid with a double track of Demerbe grooved rails, and the second from Kramat to Muster Cornelis, having a length of 4½ kilometers of single track of Vignoles rails. The first piece is almost level, with the exception of short inclines of 1 in 32 over bridges; there are two long curves, and a number of short ones of 30 meters radius. The second section has a continuous gradient of 1 in 450. The haulage is affected by 21 fireless Lamm Francq locomotives, and five stationary boilers, the whole of which were manufactured by the Hohenzollern Locomotive Works, Dusseldorf. Two of the boilers are situated at Batavia, and three at Kramat, but one only is in work at each station at a time, the remainder being in reserve. They are worked 12 hours a day, and fill an engine every 1½ minutes during about 3 hours in the day, and every ten minutes at other times. An engine charged to a pressure of 12 atmospheres will draw two or three passenger cars from Batavia to Kramat, and from Kramat to Cornelis, up and down again to Kramat. Part of the line was opened in July, 1883, and from the last annual report it appears that the cost of haulage amounted last year to 23 cents per kilometer (7.4d. per mile), composed of the following items:

	Cents.
Driving engines	4 7
Heating boilers	2.3
Coals	14.0
Packing, lubricating, &c.	2.0
Total	23.0

(5 cents = 1 penny).

More recently the cost of haulage has been only 17 cents per kilo. (5.24d. per mile), the price of coals being £2 per ton. The consumption of fuel was at first 6 kilogrammes per kilometer (21.3 lbs. per mile), but recently it has fallen to two-thirds of that amount. Repairs of boilers and engines have cost 2 cents per kilometer, and have consisted chiefly in re-turning the wheel tyres and renewing the felt on the boilers. Since the road has been completed, the receipts per month have amounted to 22,800 florins, and the total expenditure to 12,800 florins, leaving a net monthly profit of 10,000 florins (£800). The fare is 2½d. for four miles' run, or any part of it. The engines give every satisfaction. They are in native hands, and run constantly, with little attention and no breakdowns. Two more have been ordered, and will be shipped from Amsterdam this month. It is believed that with

a better road the expenses might be reduced to 50 per cent.

ELECTRIC TRAM-CAR.—A tram-car, fitted by Mr. A. Reckenzaun with secondary batteries and electro-motor, has now been running experimentally for some weeks at the works of the Electrical Power Storage Co. at Millwall.

The car is an old one, procured from one of the Metropolitan lines, and it has been drawn by horses between Greenwich and Westminster for many years. The body of this vehicle weighs 2¼ tons, and it accommodates 46 passengers. The accumulators are of a special type manufactured by the Storage Company, from the designs of Mr. Reckenzaun. Placed under the seats on long trays, which run on rollers for their speedy removal, they are out of sight, and the whole car internally and externally has the ordinary appearance. The motor and gearing—Reckenzaun's patents—are placed underneath the car, and occupy so little space that to an ordinary observer they are invisible. The speed may be varied from three miles to ten miles per hour.

The accumulators weigh 1¼ ton, the motor, gearing and accessories about ½ ton, bringing the total weight of motive power to about 1¾ ton for a car which, with its full complement of passengers, weighs itself 5½ tons; whilst the batteries, motor and gearing are capable of furnishing, at any desired moment, a power of sixteen horses, if required. This weight of motive power is compared with steam and compressed air locomotives weighing eight to ten tons, to do the same amount of useful work.

The line—4 ft. 8½ in. gauge—is 400 ft. long, forming a right angle of nearly equal sides, so that about half way a curve of 35 ft. radius has to be passed. From one end, as far as the commencement of the curve, the road is tolerably level; but with this curve commences an incline of 1 in 40, which rises gradually until it reaches a maximum of 1 in 17 nearly at the end of the up journey; thus, it is impossible to make a rush for the hill on account of the sharp curve intervening.

The running cost, including 15 per cent. depreciation on machinery and 50 per cent. on accumulators, is stated to be 3.5d. per car mile, or about one-half of the cost of horsing on tram lines. The car on the line at Millwall runs for two hours with one charge, starting, stopping and reversing every sixty seconds; and the accumulators can be replaced, it is said, almost as quickly as changing a pair of horses, by means of a trolly, which brings and removes the tray of cells, running on rollers. There are sixty of these accumulators, or thirty on each side. The load is distributed upon two small bogies, so that no objection may be raised on the part of tramway companies using light rails laid for horse-car traffic, and the old rolling stock can be utilized by putting the bogies which carry the motor under the car, and fitting the space under the seats for the reception of the accumulators. The car is lighted by four 20-candle power Swan lamps, and bell-pushes inside the vehicle enable the passengers to communicate with the conductor or driver by the ringing of electric bells.—*Journal of Society of Arts.*

ORDNANCE AND NAVAL.

THE IMPERIAL BRAZILIAN NAVY.—In a recent number of *Revista Maritima Brazileira* is given a general list of the ships of the Brazilian Navy, from which we extract the following particulars:

The largest ironclad is the Riachuelo, launched in 1883, and built by Messrs. Samuda. She has a tonnage of 5,800, is built of steel, and has steel armor 10 inches on the turrets and 11 inches on the side. Her indicated horse-power is 6,000, speed 16 knots, and she is armed with 4 Armstrong guns of 20 tons each, 6 of 5½ tons, and 15 Nordenfelt machine guns.

There are two ironclads, launched in 1876, of 3,600 tons each, named Solimoes and Javary. They are built of iron, and have iron armor 13 inches on the turrets and 12 inches on the side. Their speed is 12 knots, and they are each armed with 4 Whitworth guns of 25 tons each and 4 Nordenfelts.

Two smaller ironclads are also of iron hulls—the first, the Bahia, built in 1865, is 928 tons, has 4-inch armor on the turret, 3-inch on the side, and is armed with 2 Whitworth 7 inch guns; the Mariz a Barros is 1,196 tons, and has 4-inch armor, and carries 2 Whitworth 7-inch guns and 2 smooth bore 68-pounders. Both these vessels have a speed of 9 knots, and they are 18 years old.

A wooden ship, the Sete de Setembro, of 2,179 tons is plated with 4-inch armor, carries 4 Whitworth 9-inch guns and 4 Nordenfelts, and has a speed of 11 knots. She was launched in 1874.

There are, in addition to these, four small monitors for river service, built of wood plated with 4-inch armor, each carrying a 7-inch Whitworth gun, and having a speed of 7 knots.

As regards unarmored cruisers, the Brazilian Government has one of 4,000 tons building, of steel, to steam 15½ knots, to carry 4 Armstrong 12-ton guns, 10 small guns of 6-inch bore, and 12 Nordenfelts. They have also seven cruisers built of wood, of which four are between 1,400 and 2,000 tons, and are armed with Armstrong and Whitworth rifled cannon and Nordenfelts. Three vessels of about 750 tons each are armed with Whitworth guns. The speed of these wooden vessels ranges from 9 to 12½ knots, and most of them are of recent build.

Of vessels of smaller size, Brazil has seven wooden and five iron gunboats; and also five composite gunboats in course of construction. She has in course of construction at Jarrow, five torpedo boats built of steel, to steam 18 knots, and over 100 feet in length. Three torpedo boats are building by Messrs. Thornycroft, but of these the dimensions are not given.

Brazil provides also for the training of her seamen, a wooden brig, the Appendiz Marinhein, which completes the list.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

REPORT of New York State Survey for 1884. James T. Gardner, Director. Albany: Weed & Parsons.

The Sanitary Engineer, Vol. X.

Professional Papers of the Signal Service No. XV.; Researches on Solar Heat.

Report of Meeting of Mississippi Levee Commissioners.

Ninth Report of the Committee on the Metric System of Weights and Measures.

Annals of Mathematics, Vol. I., No. 3.

REPORT ON THE MANUFACTURE OF COKE. By Jos. D. WEEKS. New York: David Williams.

This is a statistical report prepared for the Superintendent of Census. The descriptions of coking processes in European countries are given with exceptional fullness and with abundance of diagrams.

Much space is devoted to the history of coke manufacture as a special industry, and this is substantially a history of the manufacture of pig iron, as four-fifths of all the coke made is for iron manufacture only.

An interesting book for coke or iron producers.

STEAM MAKING, OR BOILER PRACTICE. By CHAS. A. SMITH, C. E. Chicago: The *American Engineer*.

The merits of this work are its eminently practical character, and the careful presentation of those fundamental principles upon which successful practice depends.

The Nature of Heat and Properties of Steam are treated in the first chapter; then follow in order—Combustion, Externally-Fired Boilers, Internally-Fired Boilers, Locomotive and Marine Boilers, Construction and Strength of Boilers, Heating Surface, Boiler Fittings.

The illustrations are of notable examples in use.

THE SELF INSTRUCTOR IN NAVIGATION. By W. H. ROSSER. London: James Imray & Son.

The preface of this book distinctly states that the Self Instructor is essentially practical, and not theoretical.

Problems involving all useful methods in determining a ship's place are propounded and solved, with a view to aiding the student in preparing to pass the "Board of Trade" examinations.

The work would well supplement the ordinary academic course as a work for practice.

ELEMENTARY TEXT-BOOK ON PHYSICS. By PROF. WM. A. ANTHONY and PROF. CYRUS F. BRACKETT. Part I. New York: John Wiley & Sons.

This is neither rudimentary physics nor physics for advanced classes. A knowledge of trigonometry is required in the student who begins it.

The scientific accuracy of the work is unquestionable, but to be a widely useful book it needs either considerable expansion in the treatment of the topics presented, or else a more rudimentary presentation of them.

A learner who has just completed the ordinary high school course of Natural Philosophy may study this book to advantage, but he will still need a text-book on Mechanics to obtain a fair knowledge of the practical bearings of Physical Science.

CURVE-TRACING IN CARTESIAN CO-ORDINATES. By WILLIAM WOOLSEY JOHNSON. New York: John Wiley & Sons.

This little book will prove a source of delight to students who have just completed the ordinary academic course of analytical geometry.

It will doubtless serve in many instances to stimulate the reader to a further investigation of the comprehensiveness of this delightful branch of study.

The treatise will be found easy reading to all who appreciate co-ordinate geometry, and both teachers and pupils will, we doubt not, consider it a profitable supplement to the ordinary course.

THE DESIGNING OF ORDINARY HIGHWAY BRIDGES. By J. A. L. WADDELL, C. E., B. A., &c. New York: John Wiley & Sons.

The author of this work has performed a good service for the engineers who design and construct bridges for common roads.

As bridges may be bought of responsible firms, just as mowing machines are, the class of engineers upon whom the responsibility of designing a bridge would fall is probably a small one. But there is no doubt that at times such a treatise as the present one is of great value.

Prof. Waddell has with great pains tabulated the dimensions down to the smallest details. The designing is restricted to the Pratt and Whipple systems, but it is urged that the great majority of American bridges are built on these systems.

The young engineer will find the study of the book a valuable aid to designing generally.

The typography and plates are exceedingly good.

METEOROLOGICAL AND PHYSICAL TABLES. By PROF. ARNOLD GUYOT, Ph. D., LL. D. (Price $3.00. For sale by D. Van Nostrand.)

The fourth edition of this useful work, revised and enlarged, has just been published by the Smithsonian Institution. The preceding, or third, edition was published in 1859, and though stereotyped, it was thought advisable to have this new edition entirely reconstructed. It now forms an octavo volume of 763 pages (including the introductory 25 pp).

Tables that have stood successfully the test of long use by a large number of scientific workers, are well established in public favor.

References to Guyot's Tables are familiar to all readers of physical science.

The tables are arranged in series, as follows:

First series (15 in number)—Thermometrical comparisons and conversions.

Second series (of 33 tables)—Hygrometrical computations.

Third series (of 27)—Barometrical tables.

Fourth series (of 26)—Hypsometrical tables.

Fifth series—Geographical tables, including 40 of measures of length (for heights, &c.), 10 of itinerary measures, and 10 of square measures of geographical surface.

Sixth series (of 99)—Tables for correction of variations of temperature, &c., at different parts of the earth.

Seventh, and last, series—9 miscellaneous tables.

Prof. Henry, in his report for 1851 on the first publication of these tables, says:

"These tables supply a desideratum in the English language, and will doubtless be highly prized by all engaged in physical research."

In his report for 1855, in referring to the preparation of a new edition of these tables, remarked:

"No publications of the Institution have been called for more frequently than these Tables. They have been introduced into Great Britain, and have supplied a want which has long been felt by the practical cultivator of physical science in that country as well as our own."

ARCHITECTURAL PERSPECTIVE FOR BEGINNERS. By F. A. WRIGHT. New York: Wm. T. Comstock.

This is substantially Perspective without a master, although a beginner would need some preparatory instruction to enable him to solve successfully any one of the examples. The plates are good for students who are capable of beginning with the author's instructions, but then it would be an open question whether they should begin with Plate I. or Plate V. We should think the latter, unless they have acquired some previous knowledge of perspective.

Architectural draughtsmen will find valuable suggestions in this work, and as it is for such that the book was prepared we have no doubt that it will fulfill its mission.

A TREATISE ON THE ADJUSTMENT OF OBSERVATIONS, WITH APPLICATIONS TO GEODETIC WORK AND OTHER MEASURES OF PRECISION. By T. W. WRIGHT, B. A., C. E., late Assistant Engineer United States Lake Survey. New York: D. Van Nostrand. 1884. Price, $400.

This treatise will be found a valuable addition to the literature of geodetic operations; the title is, however, misleading—it implies a discussion of the various corrections required to allow for the effects of temperature, refraction, &c. Such corrections, however, are either omitted or only superficially dealt with, and the principal subject matter is the adjustment of unavoidable errors by the method of least squares.

The work commences by a discussion of the various causes of error, and several practical hints are given as to how to diminish them. A remark in connection with personal error is worth quoting: "A good observer, having taken all possible precautions with the adjustments of his instruments and knowing no reason for not doing good work, will feel a certain amount of indifference towards the results obtained. The man with a theory to substantiate is rarely a good observer, unless, indeed, he regards his theory as an enemy, and not as a thing to be fondled and petted."

In the second chapter the usual law of error is stated, and the method of least squares is deduced therefrom, together with formulæ for calculating the mean square error, the probable error, and the average error. The author points out that the name "probable error" is unfortunate, and so we think; he is also of opinion that the average error might with advantage be more used than it is at present as a measure of the precision of a set of observations. This chapter is concluded by a most instructive discussion on the laws of error, based on various assumptions as regards the number of sources of unavoidable error. It is first supposed that there is only one source of error, and that all errors between certain limits are equally probable; the curve of error then becomes a finite straight line. The next case considers two independent sources of error, the curve then becomes two straight lines intersecting on the axis of y at an angle of 45°. In the third case three sources of error are assumed, and the curve of error is shown to consist of three parts, which together form a close approximation to the usual curve of error. The method of least squares is further developed in the succeeding three chapters, and applied to the adjustment of the direct observations of one unknown, to indirect and to condition observations. Various methods of solving the numerous resulting equations are given, both rigorous and approximate; amongst the latter the method of solution by successive approximations as used in reducing the primary triangulation of the Ordnance Survey of Great Britain is strongly recommended. The author also recommends the use of a calculating machine, or of Crelle's Tables, in order to diminish the arithmetical labor.

The remainder of the work is devoted to applying the foregoing to triangulation, to base-line measurements, to spirit leveling, to trigonometrical leveling, to the graduation of line measures, to the calibration of thermometers, and to the discovery of empirical formulæ. The application to triangulation is treated very fully, and several methods of solving the necessary equations are given and illustrated by means of examples. One of these examples is the adjustment of the angles of a quadrilateral taken from the Survey of the Great Lakes of North America, executed by the United States engineers; three methods of solution are given, one of them being that adopted by the United States engineers.

The author remarks very truly that it is a waste of time applying the rigid methods of adjustment to tertiary or even to secondary triangulation, and he proposes a method of successive approximations by first adjusting the angles at each station for the local conditions, and then using these adjusted values for the further adjustment in connection with the side and angle equations of the net. It may be mentioned that the reduction of the secondary triangulation of Great Britain, now being carried out, is effected by a graphic method applied after the angles have been locally adjusted; this method is found to give excellent results with far less labor than even an approximate method of calculation. The criticism on the title of the work is well exemplified in the chapters on base-line measurements and on the graduation of line measurements. For instance, there is no mention of the corrections required to be made to a base-line measure-

ment to allow for errors in alignment or of level, for the effects of temperature and for reduction to sea level. We think that, at any rate, a sketch of these and other sources of error and their methods of adjustment would not have been amiss.

The adjustment of the errors of trigonometrical leveling is very fully considered, and one of the examples proposed for solution is the adjustment of the levels taken trigonometrically during the triangulation executed to determine the axis of the St. Gothard tunnel.

The following remark is, we think, worth quoting: "Closely allied to the preceding (elimination of accidental errors) is the common idea that if we have a poor set of observations good results can be derived from them according to the method of least squares, or that if work has been coarsely done such an adjustment will bring out results of a higher grade. A seeming accuracy is obtained in this way, but it is a very misleading one. The method of least squares is no philosopher's stone; it has no power to evolve reliable results from inferior work."

An excellent feature in the work is the illustration of the text by means of examples, embracing almost every possible case that occurs in practice. Some of these examples are fully worked out, others are proposed as exercises. Most of them are derived from geodetic work carried out in the United States. In conclusion we can strongly recommend this book.—*Nature.*

MISCELLANEOUS.

DESCRIPTIONS of some waterproof varnishes for paper are given by the *Journal of the Society of Chemical Industry* from the *Papier Zeitung* as follows:—(1) One part Damar resin; four, five, to six parts acetone are digested in a closed flask for two weeks and the clear solution poured off. To this, four parts of collodion are added, and the whole allowed to clear by standing. (2) Thirty parts white shellac are digested with 500 parts of ether, and to the solution fifteen parts of lead carbonate are added, then shaken for some time and repeatedly filtered. (3) Five parts of glue are dissolved in 100 parts of warm water, and this solution spread on paper. After drying, the paper is soaked for an hour in 10 per cent. solution of acetate of alumina and again dried, in order to give it a final glaze. (4) 120 parts of linseed oil are heated and poured into a mixture of thirty-three parts of quicklime and twenty-two parts of water, to which fifty-five parts of melted caoutchouc have been added, stirring all the time. The varnish is strained and used hot. (5) One part of gutta-percha is carefully digested in forty parts of benzine on the water bath, and the paper covered with it. This varnish can be drawn or written on.

AT the Montreal Meeting, Prof. Frankland communicated the results of a study of the phenomena attending the discharge of accumulator cells containing alternate plates of lead peroxide and spongy lead: (1) The energy of a charged storage-cell is delivered in two separate portions, one having an E.M.F. of 2 volts and upwards, the other an E.M.F. of 0.7 volt and under. One of these may be conventionally termed *useful*, and the other *useless*, electricity. (2) The proportion of useful electricity obtainable is greatest when the cell is discharged intermittently, and least when the discharge is continuous. (3) Neither in the intermittent nor continuous discharge at high E.M.F. is the current, through uniform resistance, augmented by rest. At low E.M.F., however, the current, after continuous discharge of the high E.M.F. portion, is greatly augmented, but only for a few minutes. This augmentation of current at low E.M.F. after rest is hardly perceptible when the high E.M.F. discharge has been taken intermittently. (4) The suddenness of fall in potential indicates two entirely distinct chemical changes, the one resulting in an E.M.F. of about 2.5 volts, the other in one of about 0.3 volt. (5) The chemical change producing low electromotive force is the first to occur in charging, and the last to take place in discharging the cell. It is the change which occurs during what is called the "formation" of a cell, and for economy's sake, a reversal of this change should never be allowed to take place. (6) Currents of enormous strength can be readily obtained from storage batteries coupled up in parallel, viz., a current of 55,000 amperes from only 100 cells. Such a current reduces to insignificance the output of the largest dynamo ever built. It is to be hoped that currents of this magnitude will open up new probabilities of research into the construction of matter. —*Engineer.*

REGENERATIVE ACCUMULATOR.—M. Zenger's "regenerative accumulator" is formed by surrounding the electrode forming the positive pole, with a halogen, such as bromine, chlorine, or iodine. These halogens serve to depolarize the electrode in combining, when the circuit is closed, with the hydrogen upon the cathode. M. Zenger uses bromine because of its comparative cheapness and fluidity, rendering it preferable to gaseous chlorine and hydrate of chlorine, which are very unstable, or to iodine, which is costly and of a solid consistency. The bromine is placed at the bottom of a porous vase filled with fragments of retort carbon and closed by a covering of paraffin furnished with a cork hole to insert the liquid bromine. A layer of chloride of iron is placed over the carbon; and the zinc plate is surrounded with a solution of dilute chlorhydric acid (1 to 10) mixed with 5 per cent. of glycerine to reduce the resistance of the zinc-carbon bromine cell. A very constant battery for telegraphic work, where the line has a considerable resistance, is obtained from a concentrated solution of chloride of iron, more or less diluted. In this way a hydro-electric battery of 1.95 volts electromotive force and 0.5 to 5.2 ohms internal resistance is obtained, which gives 3.9 to 0.38 amperes according to its resistance. The constancy of the pile is considerable; and the cell can be regenerated in a short time by the current from a dynamo, which effects the reduction of the bromine.